Psychological Bulletin

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Published Bimonthly by the
American Psychological Association

VOL. 51, NO. 1 JANUARY, 1954

The Psychological Bulletin contains evaluative reviews of the literature in various fields of psychology, methodological articles, critical notes, and book reviews. This JOURNAL does not publish reports of original research or original theoretical articles.

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Business communications—including subscriptions, orders for back issues, and changes of address—should be sent to the American Psychological Association, 1333 Sixteenth Street N.W., Washington 6, D. C.

Annual subscription: $6.00 (Foreign $6.50). Single copies, $1.50.

PUBLISHED MONTHLY BY

THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.
1333 Sixteenth Street N.W., Washington 6, D.C.

Entered as second class mail matter at the post office at Washington, D.C., under the act of March 3, 1879. Additional entry at the post office at Menasha, Wisconsin. Acceptance for mailing at special rate of postage under the provisions of Sec. 34.40 P.L. & R. of 1948 provided for in Section 538, act of February 28, 1925, authorized August 6, 1946. Printed in U.S.A.

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PSYCHOLOGICAL CONSEQUENCES OF BRAIN LESIONS AND ABLATIONS

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In a previous paper, the senior author (**164**) reviewed the literature dealing with the psychological consequences of organic brain lesions and ablations through the year 1941. The intervening period of over ten years has witnessed considerable research in this area. In addition to the need to scrutinize and systematize the new and extensive literature relative to the problem of therapeutic psychosurgery, it is also important to observe significant trends in the development of new psychological evaluative techniques. Also, there have been notable modifications in the use and application of existing test methods in organic brain disease.

In the previous review of the literature by Klebanoff (**164**), effort was made to formulate certain basic generalizations. It appears appropriate now to examine critically these conclusions in the light of the extensive research of the last decade. One is impressed by the striking development of new specialized test techniques. It seems important to evaluate these new techniques in terms of their potential clinical value. Research of the last decade has stressed the potential importance of such variables as the patient's age and the quality of interaction with the environment in producing differences in deficit in patients with apparently similar disease processes.

In the light of all the above considerations, the present review has been undertaken. The general attitude underlying this survey is one of critical appraisal rather than mere review; the basic objectives are to organize, integrate, clarify, and extract significant generalizations. Such an approach appears to be indicated if the basic conclusions in the area of psychological testing in organic brain disease and psychosurgery are to be evaluated in terms of essential contribution to scientific and clinical knowledge.

BRAIN INJURY IN CHILDREN

Psychological studies of persons who have suffered congenital brain injury, encephalitis, or other types of cerebral damage early in life have proven an increasingly fertile area of

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[2] This study was reviewed in the Veterans Administration and is being published with the approval of the Chief Medical Director. The statements and conclusions published by the authors are their own and do not necessarily reflect the opinion or policy of the Veterans Administration.

[3] The authors wish to express their indebtedness to members of the Clinical Psychology Service and Clinical Psychologist Trainees who have assisted in various ways in the compilation of literature.

research. The significance of research with brain-injured children, in addition to its practical value for rehabilitation, lies in the possibilities of obtaining valuable understanding of the developmental aspects of brain function or dysfunction. Theories of normal mental development (**135, 180, 286**) have stressed the fact that psychological functioning and cerebral organization show an increase in differentiation and integration with age through adolescence. Although the less differentiated structure in the child leads to a more diffuse effect of injury if it occurs early, it also makes possible maturation with compensatory development of function. This negates the influence of an injury which would frequently have serious long-term sequelae in adults.

Probst (**222**) followed up a group of 95 children with skull fractures occurring between ages one and fifteen. After seven to fourteen years there was no evidence of impairment in school or vocational achievement, social adjustment, or intellectual development. Several investigators (**50, 64, 84, 126, 163, 297**) obtained varied results in brief case studies of functions disturbed by brain injury in rather young children. French (**84**) studied ten children who were operated upon for cerebellar tumors with an extensive battery of generalized intelligence and specialized tests and found no evidence of impairment on deterioration-sensitive tests. Some children showed superior performance. On the other hand, Cotton (**64**), employed a control group in studying the thought processes of spastic children and found striking differences between them and the normal control group. It is possible that differences in severity of injury may well account for these divergent findings. An unusual case study described by Klapper and Werner (**163**) compared three children with cerebral palsy with their normal identical twins, using a large battery of tests sampling a wide variety of functions. The brain-injured children were impaired in every area when compared with their normal twins.

The major portion of research on brain injury in children has emerged in the extensive work on the distinction between endogenous and exogenous feeblemindedness. This distinction has proven of great heuristic value in stimulating an intensive study of the various dysfunctions consequent upon brain injury. As a result of this research, it has been possible to develop fairly adequate criteria for distinguishing these two types of defective children (**32, 176, 247, 256**). This has resulted in the addition of a number of fairly sensitive and important techniques to the repertory of the clinician for measuring basic functions likely to be impaired in early brain injury.

Motor Functions

The brain-injured child is generally subject to motor disturbances, since either cerebellar injuries or disturbances in the motor cortex are common (**32, 176, 247**). Even when obvious motor disturbance is not present, the brain-injured child shows awkwardness and incoordination in performance. Heath (**130, 131**), comparing exogenous and endogenous feebleminded children of comparable mental age in their performance on a simple rail-walking test, found that the exogenous children proved strikingly inferior. Descriptive studies of the motor performance of these exogenous feebleminded characterize them as hyperkinetic, overactive, restless, impulsive, and inco-

ordinated (**247, 283**). The impression gained from such descriptive studies suggests that motor difficulties are as much a function of fairly subtle disturbances in the integration of behavior and the inhibition of overt responsiveness as they are of specific damage to motor areas of the cortex. The use of the Van Der Lugt Scale (**270**) seems to offer potential for major research in the area of motor development and defect in the future.

Perceptual Functions

The most extensive and definitive studies of brain-injured children have been in the area of perception. This work has been of theoretical as well as of practical importance since it has demonstrated striking relations between perceptual and motor functioning that has led to suggestions for a sensory-tonic theory of perception (**248, 282**). Thus, Strauss (**255**), in summarizing three case histories, described perceptual and thought processes which mirror the forced responsiveness, hyperkinesis, and incoordination of the brain-injured child in his motor performance. Bender and Silver (**33**) describe the case of a brain-injured child with a severe modification in perception of the body. They conclude that the body image of the brain-damaged child is disturbed by "tonus pulls," equilibrium problems, and perceptual and integrative difficulties which heighten his social inadequacy.

Werner and Strauss (**292**) studied normal children equated for mental age with endogenous and exogenous mentally defective children without gross motor or central visual defects. They used tachistoscopic presentation of marred figures and copying of patterns of marble boards or reproduction of the patterns by drawing. Although the endogenous children were generally inferior to the normal group, they differed strikingly in mode of response from the exogenous children. The latter made a great many errors that suggested failure to discriminate between figure and ground.

Subsequent reinforcement of these findings has come from the investigations of Lord and Wood (**186**) and Dolphin and Cruickshank (**70**). The latter studied perception of embedded figures and mosaic reproduction by matched normal and cerebral-palsied children of low-average intelligence. They attributed the significant inferiority in perceptual performance of the children with cerebral palsy to forced responsiveness and meticulosity. Bensberg (**42**), in a careful study, compared familial mental defectives and brain-injured defectives using the marble-board procedure. The brain-injured group proved significantly inferior in accuracy and tended more often to show "jumps" to new lines in their drawings. The results continue to point to pathological figure-ground perception as a basic difficulty consequent on brain injury. Werner (**283**), studying the performance of the two feebleminded types on the Rorschach, noted significantly more white space responses (*S*) for the exogenous group. Case studies by Strauss and Werner (**257, 258**), and Werner and Carrison (**289**), demonstrate ingeniously the interrelationships of defective figure-ground formations in brain-injured children and other symptoms such as finger agnosia and arithmetic disability.

Similarly, these brain-injured children showed inability to grasp and retain visual patterns made up of discrete elements. The authors attributed this to a basic difficulty in integrating several aspects of a visual

stimulus. This is a defect similar to that reported for a brain-injured adult by Nichols and Hunt (**205**) and considered a basic consequence of severe brain pathology by Hunt and Cofer (**145**) in their review of psychological deficit. A case study by Schilder (**243**) has similar possibilities for interpretation of this deficit.

Brief mention may be made of additional perceptual difficulties manifested by brain-injured children. Werner and Thuma (**295**) compared equated groups of feebleminded representing the two etiological types on performance in critical flicker fusion perception. Results showed the exogenous feebleminded fell significantly below the familial group in critical flicker frequency at all three brightness levels. This finding is quite important in view of extensive research on adults reported by Teuber (**263**) and Halstead (**120**). Similarly, a marked defect in the perception of apparent or illusory motion, the phi phenomenon, and tachistoscopic presentation of stick figures in imbalance was found among the exogenous feebleminded by Werner and Thuma (**294**). These children also showed fewer movement percepts on the Rorschach than an equated group of endogenous feebleminded. In one of the few studies involving auditory perception in brain pathology, Werner and Bowers (**288**) found that the brain-injured feebleminded were deficient in discriminating melodic patterns, revealing much the same disturbance in figure-ground pathology noted above.

Thought Processes

Sorting tests have proven increasingly useful in studying thought in process and in its effect. Strauss and Werner (**259**) employed the Halstead sorting test to determine if the findings with brain-injured adults were comparable to those obtained with brain-injured children. Twenty pairs of exogenous and endogenous feebleminded children were matched for mental age and intelligence. The brain-injured children formed more relationships between objects in the voluntary sorting but accomplished this by forming "singular and unusual combinations." In choosing objects to go with rather unambiguous pictures, the brain-injured children also selected uncommon objects. They deviated more from standard names, organized objects into circumscribed units, overstressed orderliness, and showed extreme concreteness. In contrast with the endogenous children and particularly with a group of healthy children, the brain-injured children were attracted to "properties of objects apt to elicit motor responses." The latter finding seems important in view of the hyperkinetic tendencies ascribed to brain-injured children.

In a further study of thought processes, Werner and Carrison (**290**) employed a standard procedure for studying animism or the tendency to perceive natural events or objects as living. The results, based upon levels of thought development derived from the work of Piaget, showed significantly more animistic responses by the exogenous children. In keeping with certain concepts of Goldstein (**93**) and Werner (**286**), the animistic tendencies of brain-injured children may be looked upon as functions of a greater rigidity and concreteness which prevent them from detaching themselves from objects and events. They are thus unable to differentiate between their own feelings and those of the surrounding world. Evidence that these tendencies are a result of impairment due to brain injury

rather than merely representing a particular type of intellectual limitation is forthcoming in a study by Cotton (**64**), who compared physically normal children with spastic children of average intelligence. Results on a series of sorting, verbal completion, and patterning tests showed a decided inferiority for the spastics whose responses were more stereotyped and who were less inclusive in sortings and more given to highly personal associations and other signs of concreteness. These findings were particularly associated with speech difficulties and were found less often when the congenital injury was restricted to motor areas.

The problem of the rigidity or perseveration of brain-injured or feebleminded generally has been discussed theoretically by Lewin (**180**) and Kounin (**173**). The latter carried out several significant studies demonstrating differences in rigidity patterns as a function of chronological and mental age. It is not clear whether the subjects used by Kounin were exogenous or endogenous feebleminded, although apparently they were physically normal as determined by medical examination. Werner (**284**), summarizing four experiments comparing rigidity patterns of brain-injured and familial feebleminded, observed more perseveration in the former group. He characterized the rigidity of the brain-injured as *abnormal;* it involved a trend away from the global approach toward isolation of elements of a series which became self-contained and were repeated monotonously or "jumped" suddenly into the foreground. The familial feebleminded, on the other hand, showed *subnormal* rigidity related to a relative lack of differentiation. They showed predominantly global behavior, perception, or action, organized as undifferentiated wholes so that situations not sharply set apart were easily fused, resulting in stereotyped responses.

Personality

There have been comparatively few attempts at extensive personality evaluations of brain-injured children. Werner (**283**) studied the Rorschach test performance of brain-injured feebleminded and found that they showed a restriction of creative fantasy activity, strong tendencies toward explosive emotionality, negativism, and the disintegrative behavior described above. Colm (**62**), employing the Lowenfeld Mosaic Test, compared normal and brain-injured children. She described the brain-injured children as "stimulus bound," with an inability to shift and a repetitiveness which indicated an impairment in abstraction. Thus, these children showed simple additive placement of the mosaic pieces using side-by-side or color-by-color patterns and showed difficulty in formulating and executing a plan for a design. Colm indicated an important distinction between the exogenous children and compulsive or autistic children who also showed repetitiousness. The latter perseverated chiefly on words with strong emotional content while the brain-injured children repeated almost any phrase regardless of the emotional tone.

In conclusion, the extensive research on brain-injured children indicates defects that are generally like those described for brain-injured adults. The children show inability to develop abstract functions, pathological rigidity, lack of creative imagination, stimulus bondage, and disintegrative behavior in all areas. Brain-injured children also show strikingly

unequal development of capacities, suggesting some cerebral localization of damage, but the basic defects in abstraction hinder any possibility of really integrated development for many.

Organic Psychoses and Degenerative Diseases of the Nervous System

Despite the increased interest in problems of the aged in the past decade, there has been little extensive research dealing with psychological deficit or emotional changes concomitant with senescence. A major trend in the past decade has been an awareness of some decline in intellectual functioning with increased age (**151, 274**). There has been little attempt, however, at precise analysis of the nature of this decline and its relationship to emotional stability and the psychological milieu.

Fortunately, some studies on the organization of mental life in organic psychoses are beginning to appear. Botwinick and Birren (**49**), for example, compared the intellectual performance of hospitalized seniles with that of normal persons of the same age range (60 to 70 years) and with that of a matched group of young normals. They used the Wechsler-Bellevue and Babcock-Levy Revision to test for deterioration. These investigators concluded that while differences between the two aged groups existed, the older persons were far more like each other than were the normal aged and the younger group.

Of greater theoretical interest, perhaps, are studies attempting to delineate the pattern of mental change consequent on the fairly diffuse degenerative brain changes that seem to occur with cerebral arteriosclerosis and similar diseases. Thus, in studies comparing 100 male senile dements with an average age of 73, and employing such tests as Raven's Progressive Matrices, Eysenck (**72, 73**) found considerable deterioration in the patients in terms of healthy adult and child norms. A factor analysis of the results showed a general factor and three factors relating to speed, memory, and physical strength which could be identified fairly readily. Tests of abstract reasoning proved most sensitive to deterioration in contrast to tests involving memory and stable knowledge. A differential deterioration in abilities appeared to occur along the lines suggested many years ago by Hughlings Jackson (**149**) in which the diffuse cerebral damage leads to initial loss of the most complex functions which develop most recently in the evolution of the nervous system. In this connection, Halstead (**115**) studied senile dements over 70 with an extensive battery of psychological tests. There were two groups in different stages of dementia. Tests discriminating between the two groups involved the more complex functions. The tests included Porteus Mazes, Knox Cubes, Block Designs, and various recent memory tests.

The impoverishment of functioning in senile psychotics was brought out in a study by Cleveland and Dysinger (**57**). These investigators studied the performance of institutionalized seniles, whose average age was 75, with the Wechsler-Bellevue scale and an object-sorting test. The patients manifested decided difficulties in assuming an abstract attitude and could not sort on a conceptual basis. That even verbal behavior suffers impairment in senile dementia was demonstrated by Feldman and Cameron (**75**) and Ackelsberg (**3**). The former investigators compared the speech of senile psy-

chotics with that of normal adults and children. Although based on rather small samples, this study does suggest that seniles have difficulty in dealing with abstract terms and rely most heavily on concrete nominal forms or words of action to the neglect of adjectives which involve imaginative processes. Ackelsberg (**3**) obtained comparable findings in vocabulary studies of seniles. Recent studies by Pinkerton and Kelly (**217**) and Hall (**113**) contrasted deteriorated seniles with children and depressives by the use of various sorting tests and supported the thesis that a loss of conceptual ability occurs early in the course of brain degeneration while more general intellectual test performances remain relatively intact.

Diffuse degeneration of the cortex, particularly in the frontal areas, occurs in general paresis. The behavior of paretics in the classic description of the disease, while more flagrantly psychotic, does resemble that of the older senile dementia or arteriosclerotic patients. With the improved methods for treating syphilis and arresting general paresis, a re-evaluation of the description of the disease may well be necessary.

Psychological studies of patients with general paresis have been fairly frequent in the past decade and have provided suggestive findings. Studies with the Wechsler-Bellevue scale (**88, 195**) as well as with the CVS abridgement (**198**) have suggested the usefulness of these tests in delineating the intellectual performance of paretics. While there were individual differences in the degree of impairment on the various subtests, the general findings suggested lowered concentration ability, greater difficulty in new learning, verbal comprehension, and concept formation in the case of the paretic group. Trist, Trist, and Brody (**269**) used a large battery of cognitive tests in comparing normals, neurotics, and paretics and observed that the paretics found it difficult to ignore irrelevant detail, and a flowing together of figure and ground was evident in their performance.

A somewhat novel approach to the psychological study of paresis is offered in studies by Rashkis, Cushman, and Landis (**225**) and Rashkis (**224**). The former study involved the administration of a word-sorting test to normal adults, normal children, general paretics, and schizophrenics equated for educational achievement. On the basis of an analysis of the sortings, only the normal adults were able to assume the abstract attitude. Children and schizophrenics functioned at the complex, "pseudo-conceptual" level, while the paretics were unable to reach even that degree of attainment. From the standpoint of volitional attitude, the schizophrenics more closely resembled the normal adults while the paretics resembled the children. Rashkis (**224**), employing more careful controls, amplified and extended these findings. Rashkis studied three groups consisting of schizophrenics, general paretics, and cerebral arteriosclerotics. The schizophrenics proved somewhat better "coordinated" and more capable of accounting for their performance. The arteriosclerotics, although "uncoordinated," also attempted to account for their performance, in contrast to the paretics who were "uncoordinated" and offered no excuse or apology.

In one of the few attempts to study the personality characteristics of general paretics, Klebanoff (**165**) employed the Rorschach inkblots to compare findings between a homo-

geneous group of paretics and a normal control group matched for age and education. The paretics lacked sufficient intellectual drive to function at an abstract level. They showed concrete mental activity which lacked accuracy, conformity, and "fundamental adaptivity to environmental needs."

A promising and somewhat different approach to this problem is suggested by the preliminary study of Wittenborn, Bell, and Lesser (**300**) who carried out a factor analysis of symptom ratings along various scales for deteriorated organics, hebephrenics, and young patients with functional psychoses. Different symptom clusters emerged with only anxiety and paranoid factors coinciding for the groups. Deterioration was most marked among the organic patients.

Research is just now beginning to appear in connection with presumably subcortical neurological degeneration such as is found in multiple sclerosis. Although somewhat contradictory results have emerged with respect to possible mental deterioration concomitant with multiple sclerosis (**25, 53, 68**), differences in the populations studied and in the duration of the disease may account for these discrepancies. Canter (**53**), for example, found definite evidence of deterioration among patients in the early phases of the disease when compared with both their own military induction test scores and a control group of clinic employees. Diers and Brown (**68**) reported no evidence of decline, while Baldwin (**25**) reported loss of abstract ability in some patients and no apparent decline for many others.

It is in the area of personality that a striking uniformity emerges in three separate studies. A brief report by Harrower (**127**) was based on comparisons of Rorschach and Szondi findings for multiple sclerotics, control groups of normals, emotionally unstable psychosomatic patients, and patients with poliomyelitis and Parkinson's disease. It was found that the multiple sclerotic patients differed from the other groups in showing *least* concern with bodily symptoms, extreme dependency, resignation, denial of conflicts, and a need to excite sympathy. This general similarity to the classical picture of the hysteric patient with *belle indifference* also emerged in Rorschach studies by Blatt and Hecht (**48**) and an intensive study by Baldwin (**25**) of family background, premorbid factors, and data from Minnesota Multiphasic Personality Inventory patterns. The general similarity of the multiple sclerotic and neurotic hysteric is particularly striking in the early phases of the disease (**25**) and suggests the importance of a premorbid personality factor. In view of similar findings with idiopathic epileptics (**91**), an important area of research in the psychosomatic implications of neurological disease appears to require extensive exploration.

Brain Tumor, Surgical Ablation, and Cerebral Trauma in Adults

Disturbances in Perception

The majority of studies have been restricted to the field of visual perception. For excellent reviews of material on the borderline of visual sensation and perception, the reader is referred to Bender and Teuber (**40**) and Teuber and Bender (**267**). Halstead (**120**) has ascribed important theoretical significance to the critical flicker fusion frequency as a result of factor analyses of the test performances of normal individuals. On

the basis of intercorrelations between the critical flicker frequency and performance and other techniques, Halstead (**120**) has tentatively defined a power factor (P) which indicates the kinetic capacity of the individual, not unlike the "central vigilance" described by Head (**128**) in his studies of aphasia. Halstead (**117, 119**) has reported evidence that individuals of equal IQ may differ markedly in the P factor, which is sensitive to disruption by the presence of relatively small lesions in the brain and under conditions of low-grade anoxia or fatigue. Since Halstead indicates that a lowered critical flicker frequency is one of the prime indicators of a depressed level of the P factor of biological intelligence, the value of this simple laboratory procedure for psychological evaluation of the consequences of brain lesions is greatly enhanced.

Unfortunately, the findings of Halstead have not been fully confirmed. Battersby (**27, 28**) and Battersby, Bender, and Teuber (**29**) obtained negative results in studies of veteran patients with frontal lobe head wounds. These patients failed to show any significant differences in critical flicker frequency level when compared with an equated control group. On the other hand, a series of studies (**28, 29, 38, 40, 265, 266, 303**) has shown definite lowering in critical flicker frequency after occipital lobe lesions. This impairment was detected in areas that appeared normal under routine perimetric examination. Bender and Teuber (**38, 40**) have reported that defects emerged more strikingly when critical flicker frequency was tested in the periphery of vision rather than in the macular region. These results, while confirming those of Halstead (**120**) in raising doubts concerning the significance of an anatomic point-to-point projection in visual structures, are difficult to reconcile with his observations on frontal lobe patients. The differences may be a function of procedural variations or, as Battersby (**28**) has suggested, the result of differing patient groups.

Case studies comprise the bulk of the studies on perceptual response following brain injury. Rather challenging for general theories of perception are observations of a phenomenon tentatively labelled "extinction," in which patients confronted with an object in their left-half field report that it disappears as soon as another object is shown in their right-half field (**34, 36, 303**). As Hebb (**136**) has noted, these phenomena are difficult to assimilate into current perceptual theories. Bender and Furlow (**34**) describe the manner in which tendencies to reorganize a psychological field of vision about a subjective center make it difficult for a patient with occipital lobe damage to recognize that he has lost central vision.

Problems of unusual spatial reorganizations brought out only by special testing devices are described in a number of cases of occipital injury. Paterson and Zangwill (**211**) observed in two cases of right parieto-occipital lobe injuries a tendency to overestimate the distance of very near objects and to underestimate the distances of far objects, although the ability to appreciate depth and distance and the "implicit awareness of space" were not affected. Analysis of spatial structure was difficult, and various spatial relations tests and the Kohs Blocks brought out the defects in the patient. Drawings of complex objects were carried out piecemeal with poor articulation of subwholes. Temporal disorientation, also found by Coheen (**58**) with a larger number

of patients and various control groups, was also present. Since most case studies in this group represent traumatic injuries, a report by Stengel (**254**) of consequences of vascular lesions with an eclampsia in a 40-year-old woman is interesting since quite severe spatial reorganization occurred. Extensive testing revealed that for this subject the complex spatial organization of the outside world had been replaced by a most primitive organization in which "nearness" served as the only measuring scale.

A series of cases of parietal and occipital lobe injuries reported by Bender and Teuber (**38, 40**) is particularly valuable because of the careful attempts by the authors to relate findings to a general theory of brain function. On the basis of an intensive sampling of increasingly complex perceptual performances, the authors report such defects as disturbance (limited to the lower left quadrant for some patients) in localizing objects, teleopsia with a tendency toward an excess of depth in the subjective field, higher threshold for phi, lowered critical flicker frequency, and tendency to enlarge unfamiliar objects. For some patients the subjective coronal plane was rotated as a whole, away from them on the left, toward them on the right. This shifting could be compensated for by one patient in daily life, while for another, subjective visual space no longer coincided with his tactile or locomotor space and led him into many constant errors in pointing. The authors feel that these results argue against associational theories and in favor of "vector" theories generally related to Köhler's (**170**) views of brain function.

In an experimental attempt to test certain hypotheses concerning cerebral function related to Köhler's theories, Klein and Krech (**166**) and Jaffe (**150**) independently carried out recent studies of the kinesthetic figural aftereffects in matched brain-injured and control groups. Jaffe, using patients with traumatic head wounds, found no differences between controls and patients. Klein and Krech tested the hypothesis that both concreteness and figural aftereffects in the brain-injured were functions of reduced "cortical conductivity" (**174**). They predicted more pronounced figural aftereffects that would appear sooner and last longer for subjects of varied degrees of brain pathology than for equated controls. Results supported the hypotheses and, in addition, high positive correlations were found between neurologists' ratings of extent of damage and the vividness and duration of the effect.[1] The differences in results of the two studies may be in part a function of different patient populations, traumatic head wounds as against surgical ablations, a persisting obstacle to the reconciliation of data in this field.

There has as yet been little effort to relate perceptual defects in brain-injured patients to more general areas of functioning such as motivation and personality. Critchley (**66**) has stressed the relation of body image to alteration in cerebral functioning following parietal lesions. Halstead (**118**) has shown a possible relationship of lowered and fairly rigid critical flicker frequency and poor performance on the Dynamic Visual Field test to severe judgmental defects in a man following unilateral lobectomy. More recently, Weinstein and Kahn (**278, 279, 280**) have investigated motivational and interpersonal aspects of the perceptual response of 50 patients with bilateral lesions of neoplastic or vas-

cular origin. These patients showed distortions not only in body image, but in self-concepts and manifested tendencies to deny illness or impairment and to misperceive stimuli related to their disability or hospitalization. The denial of affective involvement sometimes reported following lobotomy may represent a similar phenomenon and quite a serious threat to future adjustment since it substitutes an equally unrealistic self-orientation for the previous "tortured self-concern."

Summarizing results in psychological studies of perceptual functioning following brain lesions and ablations, it is apparent that the movement toward specialized testing techniques pointed out by Klebanoff (**164**) has progressed rapidly. Perceptual research with brain-injured patients suggests that the repertory of the clinical psychologist in the examination of neurological patients may soon be augmented by the use of specialized laboratory methods such as tachistoscopic presentation, critical flicker fusion frequency testing, apparent movement thresholds, and other so-called "brass instrument" techniques. The value of these perceptual techniques in localization of lesions remains equivocal. It seems clear, nevertheless, that subtle perceptual impairments or reorganizations following cerebral injury are most clearly brought out by intensive laboratory examination rather than by the widely used global clinical methods like the Rorschach, Bender-Gestalt, Wechsler-Bellevue, Hunt-Minnesota, etc. These laboratory techniques or those outlined by Goldstein (**95**) are now chiefly available in only a few research centers (**120, 263, 307**). Before long, with the aid of these new techniques, the clinical psychologist may be in a position to play an ever more vital role in a neurological setting.

Memory and Attention

The past decade has provided comparatively little intensive research on specific memory or attention disturbances in brain-injured patients. The emphasis, rather, has been on incorporation of these functions within broader categories such as perception or general intelligence. Certainly, many disturbances of immediate memory may actually reflect difficulties in attention to presented material. The distractibility and stimulus bondage which are reported to be characteristic of both brain-injured adults and children (**93, 94, 291, 293**) would naturally impede original learning of material. Hall and Crookes (**114**) compared a heterogeneous brain-injured group with normals and schizophrenics in learning ability and found some qualitative suggestion of impairment in the organic patients, although differences between the organic patients and the schizophrenic patients were not definitive. Ruesch and Moore (**234**) studied 190 patients immediately following head injury and reported that the serial subtraction test ("100–7") proved most sensitive, suggesting that the ability to maintain sustained effort is particularly sensitive to cerebral dysfunction, whether momentary or persistent. Similarly, other investigators have found evidence of memory loss dependent largely upon failures in attention and immediate retentiveness (**4, 10, 20**). Tests such as Halstead's Formboard Retention and Dynamic Visual Field (**120**), Teuber's Field of Search (**263**), and Benton's Visual Retention (**43**) have revealed fairly clear evidence of attention and immediate memory defects in cases of brain trauma or

surgical ablation. Definitive standardization of these procedures in the future should prove them valuable clinical aids. The major problem of devising laboratory and clinical procedures capable of discriminating between certain schizophrenic patients and individuals with various kinds of brain pathology remains a difficult methodological consideration.

Mention should be made of some tests designed to detect brain damage by memory techniques (**69**, **102**, **144**). Hunt (**144**) has developed an extensive battery employing as a base the 1937 Stanford-Binet with verbal and nonverbal learning and recall tests. The test was developed on patients with diffuse brain injury, but subsequent research (**4**, **20**, **154**, **196**) suggests that the test does not discriminate well.

Employing an omnibus memory test approach, Graham and Kendall (**102**), while reporting significant differences between a brain-damaged group and a neurotic group, found impairment present in only half of the organics. In a careful study by Cohen (**59**) no measures of the Wechsler Memory scale discriminated between neurotics and patients with intracranial pathology. These negative findings do not necessarily indicate that memory defects are uncommon among patients or that memory scales are without value in neurological testing. These scales have undoubted usefulness as part of an individual testing battery in the hands of an experienced clinician. It is clear, however, that the complexity of brain functioning and the subtlety of psychological sequelae of organic brain impairment severely limit any research or clinical studies with omnibus memory scales.

General Intelligence

Although reports on general intellectual consequences of brain injuries or diseases are considerable in quantity, the past decade has seen little progress in the treatment of this issue. Critical case studies (**129**, **132**, **202**, **207**) have suggested that general intelligence as measured by standard scales may be normal or actually improve following large but clean excisions of previously pathological brain tissue. Concern in employing intelligence tests has been, with certain exceptions, limited to the Wechsler-Bellevue Intelligence Scale and various measures of deficit derived from this test. Recognizing that intelligence in general may not show a change after brain lesion, investigators, following the clinical suggestions of Wechsler (**275**), have attempted to tease out patterns of test performance capable of differentiating the brain-injured or diseased from normal individuals. Unfortunately, as Rabin and Guertin (**223**) have noted in their review of Wechsler-Bellevue research, the Mental Deterioration Index (MDI), originally developed on the basis of decline in scores with increasing age among normals, has been translated too hastily to indicate decline following organic brain damage.

A brief report on clinical cases by Levi, Oppenheim, and Wechsler (**178**) indicated, for example, the practical usefulness of the MDI in differentiating patients with organic lesions from patients with hysterical and other psychogenic disturbances. Although cautious in their generalizations, these authors did raise an important principle for use in studying deterioration. The concept of tests that "hold up" with age and hence may conceivably be less resistant to

the inroads of brain damage is a challenging one. The question remains, however, as to the specific nature of "hold" and "don't hold" tests and to the quantitative relationships necessary to suggest pathological deterioration. In its purely quantitative form, the MDI has not been shown to be sufficiently discriminating in individual determinations of brain-injured and normals (**8, 15, 47, 110, 156**).

It should be noted, however, that the general principle of the contrast of "hold" and "don't hold" tests seems to be operative and that the majority of brain-injured are properly diagnosed by its use. For individual prediction, however, reliance on quantitative scores alone seems relatively futile. Anderson (**15, 16**) compared clinically substantiated cases of focal lesions in the dominant hemisphere with comparable cases of lesions in the nondominant hemisphere. He found that while the MDI did not yield very reliable quantitative discrimination, differences were in the expected direction. Brain-injured patients with focal lesions of the dominant hemisphere were more easily picked up by the use of the index than were those with nondominant lesions. On the basis of several studies and a comparison of brain-injured and brain-diseased patients, Allen (**7, 8, 9, 10, 11**) suggested a modification of the index, but other studies have provided data which suggest that this modification is not significantly more discriminating than the original (**4, 47, 230**). Other attempts at developing deterioration indices (**138, 227**) have been evaluated by Gutman (**110**), who found that a rather complex method developed by Hewson (**138**) agreed fairly well with clinical diagnoses of brain damage.

Far more significant perhaps than the empirical derivation of complex indices (which are not very discriminating and have not been cross-validated by the investigators) is evidence that certain functions appear again and again in the list as particularly susceptible to impairment by brain injury. Almost all investigators have noted that Digit Symbol, representing disciplined psychomotor learning, is most clearly affected. Also easily impaired by brain injury are Digit Span, a test of recent memory or attention (**7, 9, 227, 233, 234**); Block Design, testing analytic and synthetic capacities (**4, 7, 9, 92, 107, 183**); and Arithmetic (**4**), testing concentration and simple numerical facility. Since these subtests are most easily affected by anxiety or other emotional disturbances, a great deal of overlap between various clinical groups (**223**) is almost inevitable.

Too much of the research in this field has been excessively test-bound and empirical, relying heavily upon omnibus scales of general intelligence. New concepts like Cattell's "crystallized" and "fluid" abilities (**54**), similar views of Hebb (**135**) related to his physiological rationale, and Halstead's four factors as components of "biological intelligence" (**120**) should provide useful theoretical bases for more precise explorations.

Halstead provided some data to suggest a differential influence of brain lesions on his four factors of biological intelligence, with the P factor considered the most sensitive and disturbed, particularly in prefrontal lobe lesions, in his data. Halstead's results also suggest a gradient of impairment in biological intelligence with the frontal areas most sensitive. This hypothesis needs extensive testing since somewhat contradictory data are available from

other studies (**29**, **40**). Nevertheless, Halstead's work poses a challenge to clinical psychologists to reconsider their techniques and their excessive reliance upon omnibus tests or techniques standardized for other purposes.

Disturbances in Thought and Language

In the past decade, research has continued to suggest that a major consequence of brain lesions and ablations is a disturbance in thought and language, classified as a loss of abstract or conceptual ability in the viewpoint popularized by Goldstein (**94**, **100**). The directions taken by research in this period have involved (*a*) more precise exploration of this basic defect by the use of refined procedures (**100**, **113**, **120**, **121**, **141**, **264**); and (*b*) attempts to ascertain whether the loss of abstract ability was particularly a consequence of frontal lobe pathology.

Extensively studied cases of both frontal and diffuse brain pathology have been reported (**2**, **44**, **118**, **123**, **205**, **306**) in which patients manifested fairly general signs of impairment in abstract behavior as measured by procedures like the Weigl color-form sorting, the object-sorting tests, the Shipley-Hartford Conceptual Quotient, the Kohs Blocks, etc.

A striking and well-studied negative case was presented by Hebb (**134**) in which the patient was operated upon for removal of frontal scar tissue that had led to near-psychotic behavior. This patient improved subsequent to a fairly clean but extensive bilateral frontal lobectomy. He showed no clear signs of unusual disturbance in abstract behavior when presented with a host of psychological tests, many of which tapped conceptual processes. Differences between Hebb's case and cases discussed by Goldstein (**98**) may lie in the nature of the injury and the possible persistence of pathological tissue in some instances.

The value of various tests of abstraction in clinical neurological testing has been brought out in a number of studies (**95**, **97**, **108**, **139**, **307**). Greenblatt, Levine, and Atwell (**108**) found that, in comparing patients with known heterogeneous types of brain damage and patients without brain damage, abstraction tests like the Kohs Block, Weigl form-color, and Shipley-Hartford were extremely successful in differentiation, and, in combination with the somewhat less accurate EEG, they could discriminate almost perfectly between groups. The tests were used clinically and the specific contribution of each is not presented, unfortunately. Similarly, Hoedemaker and Murray (**139**) noted almost perfect discrimination of 16 brain-injured from 16 schizophrenics and 16 neurotics by the use of a battery including the Wechsler-Bellevue, Rorschach, Szondi, Ellis Visual Designs, and B.R.L. Sorting tests. Inspection of their breakdown of the various functions tapped reveals that thought process disturbance (tapped chiefly by the sorting tests) and memory were most consistently found and most accurate in differentiating. Electroencephalograms were less accurate, but the test battery combined with EEG and routine neurological examination led to perfect discrimination.

Although originally devised as a nonverbal intelligence test, the Kohs Block test has proven a valuable and interesting technique in diagnosis of brain-injured patients. Wechsler (**275**) reported it to be one of the most useful subtests in his intelligence

scale for this purpose, while almost all the investigators using the Wechsler-Bellevue scale with brain-injured patients indicate severe impairment on the Block Design subtest (**4, 7, 9, 92, 107, 183**). To the extent that a combination of analytic and synthetic capacities is demanded of the subject by this test, it may measure the primarily abstract components of general intelligence. Thus, a study by Lidz, Gay, and Tietze (**183**) has shown a significant difference in mental age scores obtained from Vocabulary tests and the Stanford-Binet and the mental age scores obtained from the Kohs Blocks. A comparable group of 15 schizophrenics showed no significant differences between the three types of tests.

Significant attempts to devise more elaborate and subtle tests of conceptual or abstraction ability have been reported recently (**103, 113, 120, 121, 141, 241, 264**). A basic need in this area is a far more extensive study of the role of abstract processes in normal human behavior, and studies like those of Heidbreder (**137**) or Hanfmann (**122**) represent only tentative beginnings.

A second major problem in the area of abstract processes following brain lesion has been the question of localization. In the studies summarized by Klebanoff (**164**) the predominant trend suggested that lesions of the frontal areas in particular led to disturbance in abstract behavior. Studies by Rylander (**236**) and Halstead (**120**) particularly have supported this view. The latter investigator presented a small number of cases demonstrating greater loss of abstract ability on the category test in patients with frontal ablations than in patients with lesions in other lobes. Using a study of 147 cases of traumatic head injury, Halstead reported that the tendency these patients showed for impairment, as compared with controls, suggests again the greater effect of frontal injury, since mechanical and autopsy studies (**65, 140**) indicate that frontal injury is most likely to follow any type of head trauma. Lacking neurotic controls for his head injury group, Halstead's results are of limited generality, in view of the findings of Ross and Ross (**231**) which suggest difficulties in differentiating between these groups. The fact is that Halstead's patients with nonfrontal lesions also obtained scores considerably below those of the controls. This suggests impairment which, in view of the small number of subjects, may not be, practically speaking, less significant than findings for the frontal lobe patients. Finally, the age differences of the groups indicate that the frontal lobe patients were, on the average, seven years older than nonfrontal patients, who, in turn, tended to be somewhat older than the controls, so that age differences rather than location of lesion might account for the results. It would appear, therefore, that Halstead's views are in need of further validation.

A series of well-executed studies (**30, 264, 268**) has attempted to test the hypothesis that abstract thinking and fairly complex visual functions are most readily impaired by frontal lesions. These authors studied three well-matched groups of veterans, patients with anterior lobe traumatic lesions, patients with parieto-occipital traumatic lesions, and patients with peripheral nerve injuries who served as nonbrain-injured controls. Lesions were localized by wound of entrance. From a varied series of complex visual tasks, including the Gottschaldt "hidden figures," the

Wisconsin Sorting test (**103**), a visual-choice reaction test, and an adaptation for humans of Maier's reasoning situation, these authors concluded that test performance of groups of brain-injured was significantly below that of control subjects, despite fairly equal motivation for solution. On some tasks, the occipital lobe patients were inferior to the frontal patients. The authors feel that these results suggest that complex visual performance involving aspects of abstraction is at least as difficult for patients with posterior lesions as for patients with frontal lobe lesions.

While these results cannot be compared directly with those of Halstead (**120**) because of the difference in the nature of the pathology (trauma versus tumor), they do point up the serious consequences of any type of brain injury, particularly when it is recalled that the patients, unlike most tumor patients, were tested four to seven years after injury. On the other hand, since both the studies of Halstead (**120**) and Teuber and Bender (**268**) lack any anatomical or pathological data definitely localizing the injuries, the results have only limited value for theories of brain function. In view of the findings of Holbourn (**140**) and Courville (**65**), Halstead might well argue that the trauma of the impact of high velocity missiles in any area of the brain would most likely involve the frontal lobes in any case, thus questioning the conclusions of Teuber and Bender.

An interesting contribution to the problem of localization comes from results presented by McFie and Piercy (**192**), who studied 74 patients, largely tumor cases, with unilateral lesions. Employing the Weigl form-color sorting test, they found that patients with left-sided lesions proved significantly inferior to patients with lesions located in the so-called nondominant right side, *irrespective of location in the frontal, parietal, or occipital lobes.* This finding prevailed even when the aphasic patients were excluded. These results somewhat contradict those of Anderson (**16**) whose studies with the Wechsler-Bellevue subtests showed opposite results for patients with dominant and nondominant hemisphere lesions. Here the former patients proved inferior in verbal tests and superior in performance tests. Difficulty in comparing these results, a persistent problem because of brief reporting, makes conclusions necessarily tentative.

Personality Following Brain Lesions and Ablations

Research on personality functioning following brain lesions and ablations has been based chiefly upon the use of the Rorschach test. A major difficulty arises in conclusions drawn from the Rorschach since it is apparent that the critical factors leading to differentiation of organic patients from others derive not from general personality characteristics as much as from disturbances in thinking and perception of the types described above.

The practical usefulness of the Rorschach in studies of brain injury is emphasized in a number of studies. Aita, Reitan, and Ruth (**5**) compared 60 patients with posttraumatic brain injury with 100 controls representing a heterogeneous group of hospital patients. While quantitative analysis of Rorschach records yielded no consistent picture, use of certain of Piotrowski's qualitative signs (**218**), e.g., impotence, perplexity, repetition, and color-naming, in addition to

other signs suggesting extreme concreteness, catastrophic reactions, inflexibility, and vagueness, proved helpful. Many neurotic-like signs, depression, anxiety, and hypochondriasis were also found among the organics. Insufficient data are presented for more definitive evaluation of the discriminability of these signs, but it is clear that disturbance in the capacity for abstract thinking manifested by rigidity and extreme concreteness emerged primarily.

Koff (**168**) similarly reports the usefulness of the Piotrowski (**218**) signs in differentiating postconcussion neurotics from patients with spinal tap evidence of brain involvement. Again, differentiation is based chiefly upon those aspects of Rorschach performance which may well reflect thought and perceptual disturbances as the primary impairment. Further evidence which supports the value of a related technique calling for subjects to draw impressions from the Rorschach cards comes in a series of papers (**51, 104, 179**). Using both the Rorschach and the Graphic Rorschach, Grassi (**104**) reports ten signs generally similar to those of Piotrowski which proved most discriminating between organics and other syndromes. A distinction between blot and concept dominance in performance is made by these authors who point out the extent to which brain-injured patients are blot-dominated.

The general trend of results suggests again primarily primitive thinking and concreteness rather than a basic personality disturbance. Similar indication of a perceptual and thinking impairment is suggested by the work of Hughes (**143**), who derived a new list of Rorschach signs to differentiate organics from other groups of patients and normals by factor analysis. Another study by Ross and Ross (**231**) evaluated a number of different clusters of signs and, after extensive manipulation, developed a fairly complex scoring system of "instability" and "disability" ratings that differentiated normals from neurotics and brain-damaged patients.

The "sign" approach, as seen in these studies, seems to work fairly well with brain-injured patients, but extensive cross-validation seems essential. There has been little effort to establish the reliability of various "signs." In general, qualitative analyses of the Rorschach protocols seem to be most successful in selecting brain-injured patients. The sign approach, excessively empirical, gives little real feeling for the basic personality factors that emerge as a consequence of brain injury. It should be noted that signs described, such as impotence, perplexity, inflexibility, etc., are not intrinsic, specifically called forth by the Rorschach blots. They merely represent behavioral evidences of slowed reaction times, abnormal concreteness, perceptual disturbances, and awareness of impaired functioning. It seems that the Rorschach has thus far offered little that is new toward our understanding of the personality of the brain-damaged. However, the value of intensive Rorschach studies of individuals with brain damage is brought out by several case studies (**2, 71, 207, 306**).

Somewhat unique in the field of personality studies of the brain-injured is a research by Anderson and Honvik (**17**) comparing Minnesota Multiphasic Personality Inventory profiles of patients with frontal lobe lesions and with parietal involvement. The frontal lobe patients approximated the clinical picture of the

"hysteriod reaction type" while the parietal patients more closely resembled that of the "anxiety neurosis." Since this study lacked any normal or neurotic control groups and since no estimate of the degree of overlap is presented, conclusions remain tentative.

Summarizing the results of the meager personality studies of brain-injured patients, it would appear that in general these patients manifest abnormal concreteness, diffuse and overly generalized modes of organizing their experiences, uncontrollable emotional outbursts, diffuse anxiety ("catastrophic reactions"), and a profound sense of personal inadequacy (**1**, **5**, **104**, **148**, **155**, **179**, **306**). Lacking in the studies of the personality of the brain-injured has been any attempt to consider the personality dynamics and the adequacy of interpersonal relationships following brain damage of various types. Comparatively little effort at systematically observing social interaction patterns of the brain-injured has been reported, although some authors (**6**, **94**, **203**, **278**) have referred to problems of this sort.

Psychological Concomitants of Epilepsy

Psychological studies of patients with convulsive seizures diagnosed as epileptics have sought generally to answer two questions: Is there evidence of psychological deficit or deterioration in patients with epileptic seizures? Are there distinctive personality types or patterns of traits characteristic of epileptic patients? Since epilepsy has generally been considered a physiochemical disease (**177**), treatment has been largely by chemical means. For many years, it was felt that the chemical disturbance in the brain led necessarily to mental deterioration. By 1943, sufficient data had appeared in a variety of studies to lead Lenox (**177**) to conclude in his review that mental deterioration was by no means a concomitant of idiopathic epilepsy. A number of studies employing psychometric intelligence tests, usually the Stanford-Binet, failed to find any signs of deterioration (**18**, **74**, **245**, **252**).

Concern in the past decade has centered not only upon evidence of deterioration but upon exploring characteristic performance patterns of idiopathic epileptics with standard intelligence scales. Collins and Lenox (**61**) studied a large, heterogeneous group of epileptic outpatients with the Wechsler-Bellevue and found that the group was of better than average intelligence, probably the result of a socioeconomic selective factor in the group studied. A somewhat better controlled study by Sands and Price (**239**) compared Wechsler-Bellevue patterns of idiopathic epileptics with and without "personality problems." The groups showed average intelligence with only slight differences in their patterns.

Somewhat more definitive findings based on better controlled studies have been reported by Goldman (**91**) and Winfield (**298**). The former compared Wechsler-Bellevue patterns of equated groups of idiopathic epileptics, patients with hysterical seizures, and patients with brain lesions but without seizures. In general, the epileptic patients and neurotics showed a similar pattern and both groups differed strikingly from the patients with organic damage, whose performance was generally impaired in the areas of concentration and abstract thinking. No impairment in intellectual functioning was observed in the idiopathic epileptics.

In a similar study Winfield (**298**) compared intellectual performances of well-equated groups of idiopathic ("cryptogenic") epileptics, symptomatic epileptics with known lesions, posttraumatic brain-injured patients without seizures, and normal controls. Where possible, school IQ or military test scores were used to insure matching of premorbid intelligence levels. Brief tests of verbal meaning, spatial relations, reasoning, associate learning, and abstraction were employed. Results indicated no significant differences between the controls and the idiopathic epileptics, while the two groups with known brain damage showed significantly lower scores in all areas. As in the study of Goldman (**91**), the seizure symptom seemed less significant than the factor of structural brain damage in relation to intellectual impairment.

In general, results of recent studies of the intellectual performance of idiopathic epileptics do not provide any evidence of intellectual deterioration or of any clear-cut characteristic pattern of performance unique to these patients. The importance of distinguishing between the clearly idiopathic or cryptogenic epileptics and those who show seizures following traumatic head injury or tumor has been indicated, since in the latter group intellectual impairment does seem to occur.

A more complex problem in the psychological study of epilepsy has been the attempt to delineate a personality pattern characteristic of epileptic patients. Psychological research in this area has largely been restricted to Rorschach studies, following the original descriptions by Rorschach of a number of signs observed in epileptics. Rorschach's signs suggest a general lowered mental functioning level, poor emotional control, and difficulty in abstraction. These findings, partially confirmed by Guirdham (**109**), may, however, be a function of the use of heterogeneous, institutionalized epileptics, many of whom may have been symptomatic cases. Rorschach studies (**19**, **125**, **169**) revealed no peculiarly epileptic patterning, although tendencies toward poor emotional control were frequently observed. Lisansky (**185**), in a comparison of idiopathic epileptics and diabetics, while yielding no clear-cut pattern, pointed to slower responsivity and a greater evidence of "neurotic signs" among the epileptic population. The findings of these studies agree chiefly in the long response time observed, poor form quality, emotional constriction, and poor emotional control. In view of the heterogeneous nature of the epileptic samples, however, the findings, which strongly resemble those for brain-injured patients on the Rorschach, are difficult to interpret.

The study by Goldman (**91**) is one of the few employing well-matched groups. Goldman found no clearly defined personality pattern for the idiopathic epileptics. They did, however, show quantitative and qualitative manifestations in the Rorschach of greater drive for achievement, poorly controlled emotionality, immaturity, lowered "inner control and poise," inward turning of affect, and sexual disturbance. In general, the epileptics resembled the hysterical seizure patients more closely than the brain tumor patients in these personality characteristics. These results, while somewhat limited because of the absence of normal controls, point to a strong emotional involvement in epileptics.

PSYCHOSURGERY

Attempts to determine the functions of the frontal lobes in man, based upon studies of individuals with brain pathology, have been hampered by the lack of evaluations of premorbid behavior, the uncontrolled destruction of brain tissue, and the frequent difficulty in localization of areas affected. The various psychosurgical procedures developed may avoid these problems to some extent, but not without many seriously limiting complications. While individuals with presumably anatomically intact brains are subjected to relatively deliberate brain damage, these persons necessarily are in the midst of great emotional upheaval whether they suffer from excruciating pain, severe neuroses, or psychoses. In many cases, preoperative measures of specific functions are not obtainable. Limiting the population to accessible patients introduces a sampling bias of unknown direction and degree. When examination is possible, the reliability of the obtained data, particularly with psychotic patients, is open to question. The assessment of postoperative impairment of changes in functioning is additionally hampered by the fact that many of these persons were impaired intellectually as a result of emotional factors prior to the introduction of a brain lesion.

Essentially, frontal lobe tissue can be rendered nonfunctioning by three methods: (*a*) direct attack on the thalamus, a technique called thalamotomy or stereoencephalotomy; (*b*) cutting into the white matter and destroying varying numbers of fibers connecting the cortex and the thalamus—lobotomy, leucotomy, cortical undercutting, etc.; (*c*) excision or destruction of the cortex—prefrontal lobectomy (**215**), topectomy (**199**), gyrectomy (**212**), and thermocoagulation and venous ligation (**200**). These diverse techniques have been developed in attempts to improve upon surgical procedures and to avoid some of the undesirable personality changes accompanying the techniques as employed initially. Surgical precision, however, appears to lag far behind the specificity of the hypotheses underlying these variations. In postmortem studies Meyer and McLardy (**201**) reported great variability in the anatomical lesions produced in lobotomized patients regardless of the intentions of the neurosurgeon. In direct attacks upon the cortex, accuracy generally is not increased because of individual differences in human brains and the lack of clear differentiation among areas. Even in thalamotomy, which in theory appears to be a most precise method, additional damage may result from the gases generated during electrolysis and interference with blood vessels (**200**). The large variety of psychosurgical techniques plus the possibility of considerable variation in the extent of the lesions produced by means of any one operative procedure contribute to the difficulty of evaluating experimental findings in terms of altered functions.

Many uncontrolled factors are also at play after surgery which obscure and confound changes in functioning. Social improvement or at least increased cooperativeness is a frequently reported concomitant along with altered attitudes of clinical personnel and relatives. Attempts to organize the many and apparently contradictory findings of various investigations are difficult because of inadequate experimental designs. Samples are generally small; adequate control subjects are rarely

available; the time of testing, both pre- and postoperatively, varies considerably within as well as among studies; practice effects are generally ignored; and statistical evaluation of data is frequently omitted or inadequate. In an attempt to bring some structure into this confusion, the present review will deal as much as possible with data from specific techniques rather than with the interpretations made by the various authors.

Standardized Intellectual Tests

Table 1 presents a summary of the data in a number of studies in which standardized intellectual tests (Stanford-Binet and Wechsler-Bellevue) were administered pre- and postoperatively with quantitative data reported. Examination of the postoperative changes reveals that most of these studies show a decrement in IQ for operated patients which in several instances is quite large or statistically significant (**172, 197, 214, 238, 304**). Nonsignificant changes are reported in the others (**191, 199, 200, 220, 262, 272**).

Before evaluating these findings, practice effects must be assessed. In the first Columbia-Greystone report (**199**), Form I of the Wechsler-Bellevue was administered once preoperatively and twice postoperatively. In the second investigation (**200**), Form I was given twice preoperatively and twice postoperatively and Form II at the fifth session. While both operated and control groups manifested fairly consistent increases in IQ, the operated groups gained less (statistically not significant) than the control groups. The apparent increment in intellectual ability can thus be attributed to other factors, most likely practice. This effect also is evident in Form II of the Wechsler-Bellevue test, although to a lesser extent than in Form I. The question now arises as to the possibility that the decrements reported in the other studies might be of greater magnitude had control groups been available for comparison. Information concerning possible practice effects are particularly necessary to evaluate the McCullough (**191**) and Rylander (**238**) studies, which involved more than one retest. In retesting the same patients nine months postoperatively, Petrie (**213**) reports that the significant loss in IQ persisted, suggesting that with sufficient time interval between retests, practice effects may be minimized.

Notwithstanding the possible practice effects, all but two of the studies in which data are presented, show some decrement in postoperative performance in comparison to preoperative IQ or control group scores. Some investigators who employed other intelligence test batteries also report postoperative losses in IQ (**13, 24, 172**). These studies, while not entirely conclusive, strongly suggest that intellectual impairment measurable by means of standardized test techniques does occur following therapeutic destruction of frontal lobe tissue.

With regard to preoperative level and severity of illness, several factors stand out which should be considered in further studies. The higher the preoperative intellectual level, the more likely a large or significant decrease postoperatively (**197, 214, 238, 304**). In testing a hypothesis dealing with increased variability in the behavior of posttopectomy patients, Wittenborn and Mettler (**299**) found that ten psychotic patients with relatively high preoperative Wechsler-Bellevue scores tended to

TABLE 1

SUMMARY OF STUDIES PRESENTING PRE- AND POSTOPERATIVE IQ'S FOR PSYCHOSURGERY PATIENTS

Author	*N*	*Diagnostic Category*	*Surgical Technique*	*Test*	*Mean Preoperative IQ*	*Time of Posttest*	*Mean IQ Change*	*Remarks*
Porteus & Kepner (220)	18	Psychotic	lobotomy	SB	83.9	varied	−3.2	modified Stanford-Binet
Rylander (238)	5	Neurotic	lobotomy	SB	116.2	varied	−11.6*	calculation by these authors
Strom-Olsen *et al.* (262)	11	Psychotic	lobotomy	SB	94.3	6 wks.	−2.2	calculation by Crown (67)
Yacorzynski *et al.* (304)	1	Psychotic	lobotomy	SB WB	118 104.5	3 mos. 3 mos.	−21 −17.5	pt. received two preoperative WB's
Koskoff *et al.* (172)	5	Normals with intractable pain	lobotomy	WB	87.2	3 mos.	−20.4*	
Malmo (197)	6	Neurotic	5 lobotomy 1 gyrectomy	WB	107.8	1–3 mos.	−8.3*	WB Form II on retest
McCullough (191)	10	Psychotic	lobotomy	WB	83.4	2 mos.	2.9	mean IQ change calculated from 2nd postoperative test
Petrie (214)	20	Neurotic	lobotomy	WB	105.8	2–3 mos.	−5.0*	
Vidor (272)	21	Neurotic & Psychotic	lobotomy	WB or SB	111.5	varied	0.6	16 pts. received SB 5 pts. received WB
King (199)		Psychotic	topectomy	WB	101.7	3 wks.	3.9	surgical group gained 3.7 points less than control group
Sheer *et al.* (200)	20	Psychotic	misc.	WB	79.3	6 mos.	3.4	WB Form II on retest. Surgical group gained 4.5 points less than control group

* Significant at .05 level of confidence.

decrease more than four controls with similar initial scores. Fernandes (76), although giving no data, mentions that patients obtaining higher preoperative scores tended to drop while those with low scores tended to increase. In Table 1, the patients who obtain the higher IQ's are, for the most part, diagnosed as neurotic rather than psychotic. It seems likely that more accurate and valid estimates of intellectual ability can be obtained with neurotics, so that the impairment that occurs following psychosurgery is more readily observed in these patients. The investigation of patients lobotomized for relief from pain, moreover, reveals a large postoperative decrement (172). To what extent the severe pain interfered with optimal functioning is not known, but the postoperative relief apparently was not sufficient to overcome the loss due to brain lesion. A complicating factor in studying such populations is the fact that many of these patients are approaching death rapidly. A nonoperated control group or at least repeated examination seems essential.

With regard to the Verbal and Performance scales of the Wechsler-Bellevue, higher Verbal than Performance losses have been reported (**153, 172, 214, 304**). The differential effects on Verbal and Performance tasks, however, cannot be evaluated without a control group for comparison. Abilities measured by performance tests may be as impaired as verbal functions following psychosurgery, but the impairment may be obscured by improvement due to differential practice effects. Such improvement also makes it difficult to evaluate the permanence of any changes, although Petrie's study (**213**) does suggest the possibility of permanent impairment. Social improvement does not seem to bear a relationship to intellectual change as measured by standardized intellectual tests (**199, 200**).

Miscellaneous Cognitive Functions

In dealing with the more specific aspects of cognitive ability, such as planning ability, abstract ability, memory, learning, attention, etc., the reported findings become considerably more difficult to evaluate because of the variety of tests employed which presumably measure these functions. In examining these functions by means of the specific tests employed rather than the purported specific functions, the confusion is alleviated somewhat, and it is possible to suggest trends.

Porteus Mazes

Despite the susceptibility to practice effects of a performance test such as the Porteus Mazes, impairment below the obtained preoperative level is revealed in many studies on the *first* postoperative examination (**197, 199, 200, 214, 220, 221**). The time of the first postoperative test varies considerably among and occasionally within studies, but the immediate effects of surgery probably are not involved. Crown (**67**), reanalyzing Porteus and Kepner's and Porteus and Peter's (**220, 221**) data, reports that 23 patients whose first postoperative tests took place after three or more months show a highly significant decrement. Preoperative tests were not administered by Robinson (**83**), but a significant 3.3-year difference in favor of a socially improved nonoperated psychotic control group over the lobotomized group was found. Two studies (**153, 262**) show insignificant improvement following psychosurgery. The group Jones (**153**) employed, however, obtained almost a minimal score preoperatively.

A summary of the data of several studies in which the Porteus Mazes were administered pre- and postoperatively is presented in Table 2. Only the changes in the first postoperative examinations are given. Generally, additional examinations result in gains which eventually reach or exceed the preoperative level, but Sheer *et al.* (**200**) point out that the operated group gained less from practice than did the nonoperated controls. In the first Columbia-Greystone study (**199**), one year posttopectomy the surgical group had attained the same mental age level as the nonoperated controls. In Petrie's nine months postoperative study (**213**), the lobotomized patients did not regain the immediate postoperative loss completely, but the difference was no longer significant. Possibly the practice effect was reduced by the six-month interval between tests.

Porteus and Peters (**221**) and King (**199**) believe that a pattern of distinct loss on immediate postoperative

TABLE 2
SUMMARY OF STUDIES GIVING PORTEUS MAZES TO PSYCHOSURGERY PATIENTS PRE- AND POSTOPERATIVELY

Author	*N*	*Diagnostic Category*	*Surgical Technique*	*Preoperative Mean MA*	*Time of Posttest*	*Mean MA Change*	*Remarks*
Jones (153)	24	Psychotic	lobotomy	5.0	3 wks.	3.0	
Koskoff *et al.* (172)	3	Normals with intractable pain	lobotomy	not given	3 mos.	−4.1	
Petrie(214)	20	Neurotic	lobotomy	not given	3 mos.	−1.8*	
Porteus & Peters (221), Porteus & Kepner (220)	72	Psychotic	lobotomy	11.3	varied	−1.7*	combined data analyzed by Crown (67)
Strom-Olsen *et al.* (262)	11	Psychotic	lobotomy	12.0	6 wks.	0.1	calculated by Crown (67)
King (199)	19	Psychotic	topectomy	13.2	3 wks.	−1.2	surgical group mean MA is 2.2 yrs. below control
Sheer *et al.* (200)	23	Psychotic	misc.	10.8	10 days	−1.5	immediate preoperative MA (2nd pretest) is presented

* Significant at or beyond .05 level of confidence.

testing followed by gains up to or beyond the preoperative level in subsequent examinations is related to social recovery. This observation merits further investigation.

Abstract Thinking

Studies which report impairment on standardized intellectual tests also find impaired abstract ability (**197, 214, 238, 304**). Rylander (**238**) gained this impression from interpretation of proverbs and fables and the definitions of certain abstract words. Petrie (**214**) similarly supports this view through the use of the Stanford-Binet proverbs. In addition, however, several studies which report no over-all intellectual impairment do report some decrement in ability to think abstractly (**78, 105, 160, 199, 200**).

Studies employing the Capps Homograph test consistently report a decrement in the ability to shift. Malmo (**197**) reports a slight but definite impairment. The first Columbia-Greystone project (**199**) found a statistically significant decrease ten days posttopectomy. While most patients regained the original loss, the decrease in the ability to make verbal shifts was not regained after one year in six of the 19 subjects. In the second Columbia-Greystone investigation (**200**) the definite loss was confirmed at the ten-day postoperative test. The loss was regained at the end of 30 days owing to reacquisition of identical definitions originally given but lost postoperatively. Such studies suggest that psychosurgery results in a definite, but probably transient, deficit in verbal ability to shift.

Various sorting, grouping, and

block design tasks are less consistently reported to reveal loss due to frontal lobe surgery. Employing the Goldstein-Scheerer tests with 42 lobotomized patients, Atwell (**23**) found only slight impairment. Grassi (**105**) found that patients who showed no general intellectual impairment, evinced a temporary reduction in achievement on his Block Substitution Test. This decrement was almost completely eliminated at the end of a year. Practice effects were not considered. Freudenberg and Robertson (**85**) report a significant postoperative loss on the Kohs Blocks. The operatees also gained significantly less than did controls on a sorting test. With the modified Kohs Blocks, Weigl color-form, and the Halstead Object-Sorting tests, Kisker (**160**) concluded that some impairment does occur postoperatively.

While King (**199**), employing a wide number of measures of abstraction, failed to show any posttopectomy group changes, certain patients did show abstraction difficulties postoperatively which suggested the possibility that such deficit might be related to specific areas of the cortex excised. In the second project (**200**), postoperative deficit on a modified Weigl test at the ten-day retest was evident, with the loss regained by most patients at the three-month retest.

Some improvement on the color-form sorting test is reported by Jones (**153**), but no controls were used to determine the amount of gain attributable to practice. Hunt (**81**) and Strom-Olsen *et al.* (**262**) report no change in pre- and posttesting with the Kohs Blocks while studies employing the Shipley-Hartford consistently report no impairment postoperatively (**14, 79, 228, 262**).

Memory, Learning, Attention

Although memory and related functions have been found to be extremely sensitive to impairment associated with brain pathology in clinical situations, the reported investigations of individuals undergoing psychosurgery have not been fruitful. The gross attention and concentration difficulties characteristic of emotionally disturbed persons probably interfere markedly with preoperative assessment of these functions. Some studies simply report no gross or permanent changes postoperatively (**13, 14, 194, 238**). Extremely varied results on rote memory tasks are reported by many investigators (**24, 153, 191, 197, 228**). Thus, Freudenberg and Robertson (**85**) report significant memory loss postoperatively in a group of 24 lobotomized patients on paired associates and recall of the Bender-Gestalt figures, but no impairment in memory for objects, perhaps because of the greater concreteness of the latter task. A fairly comprehensive investigation of memory and learning was undertaken by Stauffer (**199**). Control and topectomy groups learned semimeaningful and meaningful paired-associate lists and a paragraph of verbal directions preoperatively. In general, retention of previously learned material and the ability to learn new material was unaffected by topectomy.

King (**199**) also included in his study of intellectual functions, a continuous-problem task which utilized an instrument intended for selection of pilots by the Army Air Forces. The test involved a complicated choice reaction which permitted obtaining a measure of "the patient's ability to perform on a task requiring close attention and sustained effort

over a period of time." While the topectomy group suffered no marked impairment on this task, the control group manifested a greater trend in the direction of more problems solved and fewer errors than did the operated group. Robinson (**83, 228**), testing the capacity for prolonged attention and deliberation by means of simple arithmetic problems, rhyming, and three of the Downey Will-Temperament Tests, reports clear-cut deficit in the lobotomized group as compared to nonoperated controls. Malmo (**197**) also found that deliberation was reduced postoperatively as indicated by more rapid performance on Raven's Progressive Matrices Test. Reduction in time and score on the Matrices Test as well as very rapid performance on the MMPI postoperatively was observed by Vidor (**272**). In the Columbia-Greystone project the topectomized group showed a trend toward poorer performance in addition tests, but not significantly different from the controls. A subtraction test showed significant changes in variability attributable to the operation. On a cancellation test, Rylander (**238**) reports a decrease one month postoperatively with subsequent gains to preoperative level. Hunt (**81**), however, reports postoperative increased time with improvement in accuracy on a cancellation test, as well as improvement in immediate memory.

Sensory Functions

The relief of intractable pain by means of psychosurgery is apparently afforded without increasing the threshold for pain. Using ordinary clinical tests in neurological examination of such patients, Watts and Freeman (**274**) failed to disclose any evidence of impaired sensation. Indeed, Chapman, Rose, and Solomon (**55**) report increased withdrawal reactions to pin prick are not uncommon in lobotomized patients. Following up this clinical impression and employing the Hardy-Wolff-Goodell apparatus for more accurate evaluation, they found that postoperatively their group of 23 psychiatric patients withdrew their heads from the apparatus at less intense levels of stimulation than before lobotomy. Over a two-year period, consistent trends toward a return to preoperative threshold levels were revealed (**56**). Malmo (**197**) also found a much higher rate of withdrawal after lobotomy. King *et al.* (**158**) supported these findings in a study of five patients operated unilaterally for relief of pain. In the second Columbia-Greystone study (**200**), slightly reduced thresholds were observed in only two of the nine operated patients. In this study, the radiant heat was applied to the forearm rather than the forehead.

Extensive psychophysiological studies were employed in both Columbia-Greystone projects. No marked losses or consistent changes were found in auditory acuity, visual acuity, peripheral vision, brightness discrimination, color vision, time judgment, autokinetic effect, critical flicker frequency, recognition of tachistoscopically exposed stimuli, or various motor tasks.

Personality

In his review of the clinical studies of lobotomized patients Crown (**67**) points out that "almost invariably . . . *personality changes* have followed the operation." These changes are in the direction of increased cheerfulness, complacency, apathy, restlessness, shallowness of affect, indifference to criticism and feelings of others, and decreased self-conscious-

ness, reserve, and tact. Kolb (**171**) also points out that there is little difference of opinion among clinicians as to the nature of the personality changes following lobotomy.

Rating scales have been employed in several studies (**153, 226, 244**), which generally found improved ward behavior. Cooperativeness, sociability, and tidiness are increased; anxiety, depression, and bizarre behavior are decreased. Affect and feeling are adversely affected in the direction of greater apathy (**226**). Lack of initiative is also noted (**244**). In a series of ratings, Jones (**153**) indicates that for the group, over-all behavior did not improve after the eighth week postoperatively. A time sample of behavior was obtained for surgical patients alone and in pairs by Kinder and Willenson (**200**) with no changes in patterns of behavior apparent following surgery. Patients under 40 showed some increase in activity while those over 40 manifested a decrease. Bockoven and Hyde (**106**) observed 16 patients pre- and postlobotomy in groups, recording sociograms of the patients' interactions. Improvement in psychiatric cases was generally accompanied by increased socialization and development of a friendly democratic attitude toward other patients. It is possible that increased familiarity with observers and examiners and the additional attention may lead to improvement and reduction in anxiety.

The necessity for control groups is suggested by the findings of Wittenborn and Mettler (**299**). Employing symptom rating scales, no significant differences between the topectomized and control groups were found; both groups manifested a reduction in symptomatology. Indeed, a return of certain pathological symptoms was noted in some surgical patients who had been free from such pathology prior to topectomy.

Noticeable lessening of psychotic symptomatology is reported for lobotomized patients in studies employing personality inventories (**14, 81, 199, 272, 304**). Standardized interviews utilized to obtain attitudes dealing with feelings of guilt, religion, sex, and prejudice failed to reveal any over-all attitudinal changes (**200**). Employing a sensibility questionnaire to measure degree of concern with one's past and future and with the opinions of others and a self-regarding span (a measure of the time spent talking about oneself), Robinson (**83**) compared a lobotomized group with a nonoperated control group and found that the operated patients showed less self-preoccupation, less concern with the past, future, and opinions of others. Thus, for the most part the feelings and attitudes expressed by patients undergoing psychosurgery are in accord with the clinical observations and ratings made of their behavior.

In summarizing the Rorschach data for the first Columbia-Greystone project, Zubin (**199**) points out that some subjects showed altered personality trends, but that no definite patterns of changes emerged. Analysis of group changes revealed a pronounced decline in reaction time for the operated patients. Suggested trends were posttopectomy decreases in number of responses and factors primarily associated with anxiety, ambitiousness, conflict, introspection, and perceptual accuracy. Atwell (**23**) also reports increased constriction, perseveration, and stereotypy along with less spontaneity, initiative, and fantasy. The most marked postoperative change in Atwell's study was the patient's carefree and uncon-

cerned approach to the Rorschach. Similar changes in Rorschach factors also emerged in studies employing fewer subjects (**86**, **304**). Jones (**153**) found an increase in responses, but taking into account the initially constricted record and possible practice effects of repeated testing, these results do not contradict the abovementioned studies.

Some retest changes apparently may occur regardless of surgery according to Wittenborn and Mettler (**299**). Employing a novel Rorschach measure (lack-of-perceptual-control score), revealed by responses in which form is absent or secondary to color or shading, they found a significant difference between topectomized patients and controls. Topectomy patients increased in "lack of perceptual control" postoperatively while control patients showed a decrease in this measure.

In a comparison of pre- and postlobotomy Rorschach records of 40 patients, Hunt (**81**) reports greater constriction two weeks postoperatively. Somewhat contradictory to the other studies are findings of increased populars and testing time and decreased perseveration and self-references. The patients also manifested less reluctance in their approach to the task, less self-criticism and concern over performance. Amaral (**13**) reports an almost identical picture postoperatively for 18 patients.

In a study on creative ability, Hutton and Bassett (**147**) indicate that the Rorschach, the Harrower-Erickson Multiple-Choice Rorschach, a story-telling, and a drawing test reveal a lessening of creative ability in leucotomized patients. Few patients, however, manifested a creative urge prior to the operation. In a later study, Ashby and Bassett (**21**) employed a drawing test and found no difference between operatees and psychotic controls. Both patient groups did worse than normal control subjects.

Ashby and Bassett (**22**) studied the psychogalvanic response of 21 lobotomized patients and 21 controls to real and symbolic (unfulfilled) threat. Six patients were also studied pre- and postoperatively. No uniform postoperative trends emerged; the symbolic threats retained the power of eliciting the psychogalvanic response. Thus, the authors conclude that emotional drive is not diminished by lobotomy.

Petrie (**214**) reports a postoperative decrease in neuroticism as manifested by reduced body sway suggestibility and a smoother work curve in a neurotic group. Diminished introversion was also evident as revealed by loss in persistence, a tendency to go for speed rather than accuracy, reduced self-blame, greater reality adjustment on level-of-aspiration tests, and a tendency to live in the present.

In general, personality studies do suggest a decrease in depression and anxiety following psychosurgery. These changes seem to occur at the expense of greater personality constriction, decreased critical standards and regard for others. Evaluation of the presence of "organic" indicators is obscured by their presence in preoperative Rorschach records (**161**, **271**) or by the overlap with postoperative psychotic residuals.

Conclusions

The last decade has witnessed a significant change in the orientation of research in the field of organic brain dysfunction. Klebanoff (**164**), in his earlier review, had concluded that psychological studies in this field had been concerned primarily

with relating mental functions to localized areas of the brain, the determination of the presence of brain pathology on the basis of psychological test performance, and the definition of an "organic psychological syndrome." Methodologically, essential reliance had been placed upon the use of omnibus test techniques such as the Stanford-Binet and Rorschach. However, the beginning of a trend toward the use of more specialized test techniques had been observed.

The period covered by the present review reveals decreased emphasis upon localization and diagnosis and a concomitant increase of interest in the developmental aspects of brain injury, emphasis upon patterns of functioning through the use of specialized tests including laboratory methods, and an overly hasty concentration of effort upon the psychological consequences of psychosurgery.

During the past ten years, increased theoretical sophistication and research findings have served to dispel the earlier optimism concerning the ability of psychological test techniques to contribute toward localization of brain pathology. The correlation of psychological test performance with specific areas of brain damage has been found to be limited by vast differences in brain pathology caused by different types of injury or disease as well as by serious limitations in the techniques of anatomical localization. In general, psychological instruments have proven incapable of differentiating patients with presumptive injury to specific cortical areas. Indeed, the limited number of studies which do report such differentiation merit repetition or cross-validation, an apparent constant necessity in this field of research.

The continued development of specialized test methods reflects the inability of conventional psychometric techniques to reveal clear pictures of organic impairment. Thus, the considerable research employing the Wechsler-Bellevue scale reveals that although the test as a whole proves relatively insensitive for diagnostic purposes in patients with brain injury, certain of the subtests appear to be quite discriminating in numerous studies. In addition, the trend toward the use of laboratory perceptual techniques, such as the critical flicker fusion, the phi phenomenon, and the tachistoscopic presentation of stimuli, have opened additional horizons for study and merit concerted research exploration. These methods do reveal promise, but one is not yet able to evaluate their ultimate significance as differentiating techniques.

A further significant development in recent years has been the rather extensive research upon children with organic brain disease. Analysis of the deficit findings in children with brain damage reveals that the impairment tends generally to parallel that observed in adults with brain damage. There is, however, the definite suggestion that children with brain pathology manifest marked unevenness and inconsistency in the development of their intellectual capacities, and this may indicate more generalized cerebral localization of function in children. The research directed toward differentiating endogenous and exogenous feebleminded children has suggested promising new psychological approaches and techniques in addition to making interesting theoretical contributions.

Finally, it is felt that subsequent research in this field should recognize the importance of the interaction of such related variables as environment

and premorbid personality of patients with organic brain damage. It is apparent that intellectual deficit must be evaluated in relation to the richness and complexity of past and present environmental situations. Indeed, the tempo and degree of so-called deterioration in certain senile and organic conditions may be determined significantly by the nature of social and other environmental factors. In addition, numerous questions arise that emphasize the need for additional research dealing with the matter of personality changes believed to be associated with organic brain disease. For example, do existing intellectual limitations present the appearance of fundamental personality alteration or are there basic qualitative changes in the premorbid drives, motives, and basic personality dynamics of patients following organic brain insult?

It is unfortunate that most studies of personality changes in organic brain disease have utilized the Rorschach test and reliance upon test signs which have not been adequately cross-validated. A more fruitful evaluation might involve an approach aimed at an analysis of the particular social, familial, and personal demands made upon the patient with organic brain damage. Following such an analysis of expected and desired behavior, specialized psychological techniques might be employed to evaluate the capacities required to fulfill these environmental demands. Such an approach would appear to offer extremely vital information concerning the consequences of brain injury or ablation. At the present time, there is a striking absence of psychological research designed to understand the altered dynamic field of the adult with brain injury. Similarly, there is a need for scientific investigation of the relationship between such variables as premorbid personality and socioeconomic and cultural milieu in relation to mode of adjustment to brain disease.

When one considers the literature dealing with psychosurgery, the results prove generally to be disconcerting. Although inconsistent and paradoxical results are observed when identical test techniques are employed, there are some trends in the results reflected in Tables 1 and 2. The varied conclusions in this area of research appear to be a consequence of numerous kinds of errors. First, psychosurgery is an extremely broad term and subsumes a large number of different surgical procedures involving varying degrees of destruction of brain tissue. Second, when psychometric and psychological test techniques are utilized with severely psychotic patients, the reliability or representativeness of the results may be questioned. Third, the results in the majority of the studies are not comparable since different or heterogeneous diagnostic groups of patients were employed. Finally, a large number of studies dealing with the effects of psychosurgery have been marked by faulty experimental design, particularly by absence of the use of adequate control groups.

Despite the difficulties cited above, it is possible to extract some generalizations regarding the impact of therapeutic psychosurgery. For example, in those patients whose preoperative test performance was not markedly disturbed, there is some suggestion of impairment in general intelligence, abstract thinking ability, memory functioning, learning ability, and sustained attention. Qualitative evaluation of personality changes appears to indicate a more apathetic, less complex, and constricted individual

who shows less introspective concern with himself and less depression of mood following lobotomy. It is clear, however, that experimental knowledge of the sequelae of psychosurgery remains restricted and conflicting. Continued research is necessary in this field with subsequent emphasis upon experimental design and the use of control groups.

The present review has attempted to organize and integrate the extensive literature dealing with organic brain damage over the past decade. The voluminous body of literature covered reflects adequately the degree of scientific preoccupation with this area of research. It appears that the change in emphasis from mere diagnosis and localization in the direction of the study of related variables represents a scientifically healthy reorientation. The introduction of laboratory test methods represents significant continued exploration of the utility of specialized techniques. It is felt, finally, that future research upon the patient with organic brain disease should regard him as a complex individual whose social, economic, and intellectual environmental demands must be considered in order to attain total understanding of the specific consequences of brain pathology.

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Received February 11, 1953.

THE PSYCHOLOGICAL BULLETIN
Vol. 51, No. 1, 1954

INTELLECTUAL FUNCTION OF THE TEMPORAL LOBES[1]

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The clinical literature on the intellectual effects of human brain damage reveals a constant preoccupation with the problem of the role of the frontal lobes, with a corresponding neglect of other parts of the cerebral cortex. In particular, there is not a single systematic investigation of the effects of temporal-lobe damage in man, although there are several isolated and highly suggestive reports of individual cases. Fortunately the situation is quite different with regard to animal work, which in the last few years has yielded numerous reports dealing with the effects of temporal-lobe lesions of varying extent on the learning ability of lower primates. This material draws attention to types of deficit which might well be found at the human level also, but which have been neglected; for this reason the animal data will be presented in some detail, before passing to a review of the clinical literature.

Since this review deals only with cognitive functions, there will be no detailed discussion of the emotional changes often seen in temporal-lobe damage. Certain salient facts should, however, be noted. In the monkey, a decrease in emotional reactivity regularly follows deep-temporal removals (**7, 17, 60, 80, 88, 105**); in man, electrographic abnormality in the anterior temporal region is frequently associated with personality disturbances (**4, 81**), and ablation of the temporal poles may cause extreme docility (**38**). It is now believed that such phenomena depend largely on damage to the amygdala, as removal of this area in animals has striking, though inconsistent, emotional effects, causing savageness in the cat (**8, 101**) and tameness in the monkey in the studies mentioned above. The inconsistency is puzzling, but MacLean (**74**) points out that Schreiner, Kling, and Galambos have recently described increased docility in cats also after bitemporal excisions, although this does not appear in the published abstract (**97**). Stimulation of the amygdala in waking animals has also been carried out by Gastaut (**35**) for the cat, and by MacLean and Delgado (**75**) for both cat and monkey, and here there was no contradiction, well-organized rage responses being elicited from both species.

Such observations form part of broader inquiries into the role of the phylogenetically more primitive parts of the cerebrum in emotion. Thus Gastaut (**35**), in an admirable review of recent work on the rhinencephalon, points out the rich connections and structural complexity of the amygdala, which could well make this a key area in any emotional regulatory system. This literature has also been surveyed by MacLean (**73, 74**), but with theoretical speculations which are perhaps more elaborate than the data warrant. Further discussion would be out of place here.

[1] This study was supported in part by the National Research Council of Canada (Grant A. P. 17). The author gratefully acknowledges help from Dr. Wilder Penfield and the staff of the Montreal Neurological Institute, and from Dr. D. O. Hebb.

Review of Animal Studies

The cognitive effects of temporal-lobe damage have been most clearly demonstrated in animal studies. A series of careful ablation experiments on the posterior cortex of the monkey has shown conclusively that the temporal lobes have important visual functions, and indeed that a major extrastriate focus for vision is to be found in this region. The experimental evidence also suggests that the temporal lobes play an appreciable, though minor, role in tactual discrimination.

Vision in Monkey after Removal of the Temporal Lobes

The first evidence that temporal-lobe lesions affect visual functions appears in the work of Sanger Brown and Schafer (**17**), who in 1888 reported gross, though transient, disturbance of visual object recognition in monkeys after removal of the temporal lobes. However, the importance of this finding does not seem to have been realized at the time, and no formal testing was done. Recent interest in the topic stems from Klüver and Bucy's (**60**) dramatic demonstration in 1938 that destruction of the temporal lobes in the macaque brings about a state of "psychic blindness," which they believed to be similar to that of visual agnosia in man. Their animals no longer discriminated between edible and inedible objects, nor between neutral and previously fear-provoking ones, and were markedly handicapped on tests of form discrimination; yet these disturbances could not be attributed to sensory loss, since the animals showed only slight upper-quadrant field defects and were abnormally reactive, rather than unresponsive, to visual stimuli.

One interpretation (**33**) of these results attributes the deficit to accidental damage to area 19, areas 18 and 19 being traditionally regarded as the visual association area, the seat of higher visual functions. However, the evidence is strongly against such an interpretation. Attempts to duplicate Klüver and Bucy's findings by extensive lesions within the prestriate cortex have failed. Large selective ablations of either the lateral or the mesial surfaces of areas 18 and 19 can be made with only slight visual disturbance (**19**, **64**); and it has been claimed by Chow (**21**) that destruction of all but the inaccessible posterior ventromedial sector of the prestriate region only retards visual learning in so far as the animal may have to adjust to an excessively restricted visual field. As against these negative findings, the evidence for visual deficit following prestriate ablation is meager and internally inconsistent: Chow (**19**, **21**) and Lashley (**64**) failed to confirm Ades's (**1**) report of amnesia for visual habits following one-stage bilateral prestriate ablation. Lashley's own finding of deficit on a visual conditional reaction has not been corroborated by Chow (**21**), and disturbance of object recognition in the first few postoperative days was seen by Chow but not by Lashley. According to Riopelle and Ades (unpublished study, cited in **92**) there may be transient retardation of new learning, but there is no residual deficit. It is clear that the picture of mild, transient, and variable disturbance which emerges from these studies discredits the traditional view that the prestriate cortex is the major visual association area. Coupled with Klüver and Bucy's findings, it directs attention to the temporal lobes as a probable extrastriate focus of visual functions.

Klüver and Bucy's temporal-lobe extirpations were large, and included

the hippocampus and amygdaloid complex bilaterally, sparing only the primary auditory area (and most of the optic radiations). Later work has confirmed that such extensive damage to temporal neocortex and allocortex suffices to cause profound visual disturbance, even though care is taken not to invade the prestriate region (**12**, **80**, **88**, **90**, **91**). The disturbance has tended, however, to be less dramatic than that described by Klüver and Bucy, and only Mishkin (**80**) has reported persistent loss of object recognition, which is of course the essential feature of visual agnosia in man. In formal testing situations all animals show impairment on a variety of visual discrimination problems after removal of the temporal lobes; the deficit is apt to be particularly severe on tests of pattern discrimination but there may also be disturbance of color (**12**, **19**, **80**), and even of brightness discrimination (**19**, **80**).

When attempting to evaluate the effects of temporal-lobe lesions, two points should be emphasized. First, the behavioral changes cannot be attributed to the slight and variable visual field defects that have been found, since much greater field defects may occur after parietal-cortex removal, but without producing such disturbance (**12**); the same is true in the case of extensive subtotal damage to the striate cortex (**40**, **59**, **98**). Secondly, the effects cannot be attributed to general intellectual deficit. The attempt to limit visual functions to the striate cortex, while ascribing visual deficits seen with extrastriate lesions to a nonspecific loss (**65**, **66**), simply does not fit the facts. An animal may succeed on auditory or somesthetic problems at a time when it shows severe visual loss, and there is evidence that temporal-lobe damage need not prevent good performance on delayed-reaction problems (**52**, **80**), although in some cases it apparently does (**12**). Thus we are forced to conclude that temporal-lobe ablation has a selectively detrimental effect on visual learning and retention.

More recent studies have sought to clarify the nature of this effect. Chow (**20**) has been able to show that the loss of previously learned visual discrimination habits after temporal-lobe removal is not due to the destruction of specific memory traces, since in one out of two monkeys further training on other discrimination problems reinstated the original habits. (The training is considered important, since no such dramatic recovery occurred in any of the three control animals.) But it is doubtful if anyone would defend the view that temporal-lobe ablation primarily destroys specific memory traces. On the contrary, most emphasis has been laid on the difficulty of new learning (**60**, **80**, **90**). In view of this, it is surprising to find Chow stating that there was no retardation of new learning, especially as one experimental animal completely failed to learn a new, or to relearn an old, pattern discrimination. It is true that the deficits he reports are in general mild, but this may well be due to the fact that his lesions spared the ventromedial portion of the temporal lobes, an area which, as we shall see later, is particularly important for visual functions. It should be added that the small number of animals and the marked individual differences in performance further limit the inferences which can usefully be drawn from this study.

Chow's own conclusion "that the temporal neocortex of monkey is not a storehouse for visual memories, but that it exerts some facilitative influence on the neural substrate for visual

discrimination" (**20**, p. 436) is cautious enough. Lashley (**66**), however, has gone a step further in citing Chow's data as evidence against the view that the temporal lobes have a visual function. The argument does not seem convincing. It rests on the assumption that if there is no loss of certain specific visual memories and no clear-cut sensory defect (such as a restricted visual field or a total loss of color vision), then we must be dealing with some quite general impairment, such as a loss of comparison attitude or a lowering of "vigilance." Such a view does scant justice to the complex nature of visual perception; it also ignores clear experimental evidence that temporal-lobe ablation does not affect all discrimination learning equally, but produces a marked deficit only on visual problems.

This deficit is admittedly hard to define. A visual disturbance which affects brightness, color, and pattern discrimination, while leaving object recognition and visual acuity intact, cuts across the classical distinction between complex "associative" functions and simpler sensory ones, and thus lends support to those who, on the basis of systematic studies of human brain damage, doubt the existence of pure disorders of visual integration (**9**, **10**). It has been pointed out that we cannot decide intuitively what constitutes a simple, and what a complex visual task (**19**, **80**). If complexity is measured in terms of difficulty for the intact animal, then Mishkin (**80**) finds, and Riopelle and Ades (**90**) agree, that, other things being equal, the degree of retardation on a series of visual discrimination problems after temporal-lobe ablation varies directly with the complexity of the problem. Yet we know that this is not so for all types of visual problem. Riopelle and his associates (**91**), using patterned-string problems, found that the overall level of performance was not significantly changed by removal of the temporal lobes, but that the order of difficulty of the problems was not the same for the normal as for the operated monkeys. The data suggest that certain configurations which are perceptually misleading for the normal animal become less compelling after destruction of the temporal cortex.

The fact that the same operated monkeys failed to form learning sets when tested with a series of different object-discrimination problems is a new and interesting finding (**91**). Apparently the monkeys found the three hundredth problem as difficult as the first, although some progress was always made on the six trials allotted to any single problem. These observations are clearly important, but interpretation is complicated by the fact that the operated animals never reached the same level of proficiency as the normals on the individual problems, so that the interproblem transfer effects for the two groups cannot be compared fairly.

Visual Localization within the Temporal Lobe (Monkey)

The experiments just discussed were undertaken in order to define more precisely the nature of the deficit seen after destruction of the temporal lobes. The question next arises as to whether a comparably severe visual disturbance may be produced by smaller lesions within the massive temporal cortex, or whether the only important factor is the amount of temporal-lobe tissue destroyed.

Reviewing the literature with respect to the lateral surface, we find some inconsistencies. Klüver and Bucy (**61**) emphasize the lack of gross visual disturbance following bilateral removal of either the first, sec-

ond, or third temporal convolutions, and Ades and Raab (2) report that damage to the lateral surface had no effect on a learned pattern discrimination (they do not report the extent of the damage). Similarly, Mishkin (80) presents essentially negative results from lateral-surface extirpations, although he finds retardation in the mastery of new and difficult discrimination problems. However, Chow (19) describes somewhat more extensive impairment; after lesions in the region of the middle temporal convolution his monkeys showed deficits in retention and in subsequent reacquisition of various visual discrimination habits.

The effects of damage to the ventromedial surface of the temporal lobes are more consistent and more striking. The most systematic behavioral study is that of Mishkin (80) who reports that animals are as handicapped on formal tests of visual discrimination after ventral-surface extirpations as they are after complete removal of the temporal lobes, the only difference being that with the smaller lesions the visual disturbance is not apparent to casual inspection. Persistent impairment of form discrimination after damage to the "baso-medial" cortex of the temporal lobe is also reported by Poirier (88), thus confirming the importance of this area for visual functions.

A further problem of localization is raised by the suggestion that the peculiar effectiveness of deep temporal-lobe lesions may be due at least in part to destruction of the hippocampus (2, 60, 64). The hypothesis has been refuted by recent work. Mishkin (80) has data which indicate that where visual impairment follows removal of the hippocampus it is due to cutting of the white matter in the ventral surface of the temporal lobe, and not to destruction of the hippocampus per se. The fact that severing the fornix, and so interrupting the long projecting pathways of the hippocampus, has no effect on visually guided behavior (34) strengthens this interpretation.

The experimental evidence presented so far has established the special importance of the ventromedial part of the temporal lobe for visual discrimination; the lateral section is implicated to a lesser degree, and minimal disturbance follows damage to the prestriate cortex. But this account is incomplete, in that it is based on the effects of circumscribed lesions of temporal lobe or prestriate cortex. There is also evidence to show that posterior-cortex ablations which involve combined destruction of the prestriate and lateral-temporal neocortex are in fact exceedingly damaging to visual function.

Lashley and Clark (67) point out that there is no anatomical justification for subdividing the posterior association area of the macaque neocortex into separate parietal, temporal, and preoccipital regions. It is also known that only minor behavioral changes result from isolated destruction of any of these areas (2, 64, 94). On the basis of such facts Blum, Chow, and Pribram (12) argue that the posterior neocortex, exclusive of sensory projections, should be treated as a unit, and that, as such, it must be ablated in its entirety if we are to discover its true functional significance. They accordingly made extensive lesions on the lateral aspect of the posterior cortex, involving damage to parietal, temporal, and preoccipital regions, and thereby produced a very marked loss of discriminatory ability. It is remarkable that such lesions cause at least as much visual impairment as does complete removal of the temporal lobes, despite the fact that the important

ventromedial area of the temporal lobe is left intact. This finding has since been amply confirmed (**92, 107**); it is also consistent with an earlier report of permanent loss of a pattern-discrimination habit after combined destruction of the lateral-temporal and prestriate regions (**2**). It should be emphasized that these severe and persistent disturbances of discriminatory function do not automatically accompany any large cortical lesion, but are specific to bilateral posterior-cortex extirpations; only mild and essentially transient effects follow bilateral prefrontal or extensive unilateral ablations (**92, 107**). It is interesting that very similar conclusions have recently been reached concerning complex visual functions in man (**104**).

To recapitulate: it has been established that in the macaque visual learning ability depends on the integrity of a small area of extrastriate cortex, the ventromedial surface of the temporal lobe, while no small lesion elsewhere is crucial. Much larger destructions of lateral temporal surface and preoccipital cortex have equal or greater effect. It appears therefore that facilitation from the lateral-temporal-prestriate complex is required for the functioning of the ventromedial focus, or, as an alternative statement, that the two form a single system in which the smaller ventromedial area has a special importance.

Somesthetic Functions of the Temporal Lobe

While the ablation studies described above leave no doubt as to the importance of the temporal lobes for vision, the evidence is far less conclusive in the case of somesthesis. It is true that Klüver and Bucy (**60**) inferred from the behavior of their animals that they were unable to recognize objects tactually after removal of the temporal lobes, and thus that their "psychic blindness" was not exclusively visual. There was, however, no loss of cutaneous sensitivity, and unfortunately no complex tactual and kinesthetic tests were given. At first sight the suggestion that temporal-lobe damage may interfere with somesthetic functions is a little disconcerting, since the parietal lobe is the traditional locus of such functions, and the somesthetic importance of the posterior parietal lobule has long been recognized (**94**). Recently, however, Blum, Chow, and Pribram (**12**) and Blum (**11**) have shown that extension of parietal lesions into the posterior cortex of the temporal lobe causes a greater somesthetic impairment than do posterior parietal lesions alone; the loss was measured by means of an extensive test battery which included tactual latch-box and form-discrimination problems. These studies also show conclusively that in the monkey temporal-lobe ablation by itself is essentially without effect upon such tests; the most that could be demonstrated was a slight difficulty in the discrimination of roughness (**12**). It follows from these two findings that the temporal lobe is not focally concerned in tactual and kinesthetic functions, but that it can play a minor role, at least when the more important parietal area is excised.

Blum (**11**) has already commented on the similarity between these findings and those of Evans (**28**) on tactual and kinesthetic deficit in cases of human brain damage; both human and animal studies suggest that damage to cortex posterior and inferior to the acknowledged somesthetic area of the posterior parietal lobule increases the sensory deficit. There is also evidence that greater and more enduring somesthetic deficit results from cortical ablations in man than

in chimpanzee, and in chimpanzee than in monkey (**11, 94**). These facts combine to suggest that some somesthetic loss may be expected in man after removal of the temporal lobes, even without parietal injury.

The Physiological Problem

As has been seen, behavioral studies show beyond doubt that the temporal lobes mediate important visual functions; the question therefore arises as to the nature of the central nervous pathways involved. The first problem is to locate conduction pathways from the visual receiving area to the temporal lobes, which are able to survive extensive damage to areas 18 and 19. Data will be presented which argue strongly against an explanation in terms of transcortical systems alone, and which emphasize rather the role of the pulvinar in higher visual functions. A further problem is posed by the fact that similar visual deficit results from either small lesions on the ventral surface of the temporal lobes, or from much larger removals on the lateral surface of the posterior association cortex.

Strychnine neuronography has distinguished three zones within the temporal cortex of the macaque, and these differences appear to be correlated with differences in function. The three subdivisions are: the temporal pole; the supratemporal plane, together with the cortex on the superior lateral surface of the temporal lobe; and the main temporal sector comprising the ventral and posterolateral portions of the temporal lobe. It is this third section only which has been implicated in visual functions, and its organization is therefore of paramount interest here. What is known of the other two temporal regions may be briefly summarized. The temporal pole, an anatomically distinct region, has connections with many areas, notably the orbitofrontal cortex and the amygdala (**68, 89**). It is chiefly concerned in autonomic and related somatic-motor functions (**55, 75**), though recent electrophysiological studies reveal diffuse connections between the temporal tip and the posterior association nuclei of the thalamus, suggesting that the polar region has more in common with the main temporal sector than is generally supposed (**3**). The second region, that of the supratemporal plane and adjacent cortex, contains the projection field of the medial geniculate, and subserves audition. Thorough studies have been made of its transcortical and intracortical connections (**5, 102**).

The main temporal region has many connections with other cortical areas, the most suggestive from the standpoint of visual function being those to and from the prestriate cortex. Such connections have been found for both the posterior lateral and the inferior temporal regions (**6, 77, 87**). In contrast to this it has generally been believed that there are few thalamic projections to this area (**106**), but such a view is no longer tenable. Both Chow (**18**) and Simpson (**99**) have provided anatomical evidence for projections from the posterior part of nucleus pulvinaris medialis to the lateral surface of the temporal lobe, and Chow has also confirmed earlier reports of projections from the posterior part of nucleus pulvinaris lateralis to the posterior temporal cortex. Furthermore, Pribram and Mishkin[2] have now found that, in the baboon, combined ventral surface and hippocampal lesions produce degeneration in the ventral half of nucleus pulvinaris inferior and also in the postero-

[2] Mishkin, M. Personal communication, 1953.

ventral half of nucleus pulvinaris lateralis. This is taken to indicate that Chow's original description of these projections, which he placed in the lateral temporo-occipital area, should be extended ventrally. We see, then, that both the inferior and the posterolateral portions of the temporal lobes are rich in transcortical and thalamocortical connections.

In trying to elucidate the role of this main temporal area in visual functions, we run at once into the following difficulty. It is known that the striate cortex (area 17) has no direct connections with the temporal lobes, but fires locally into area 18, which thus acts as a way-station for transcortical impulses from the primary receiving area (**14**, **22**). Yet we have seen that extensive damage to areas 18 and 19 does not interfere greatly with visual learning; particularly striking is Evarts' (**29**) demonstration that extensive ablation of area 18 has no effect on either retention or postoperative learning of a "conditional" problem requiring the association of visual and auditory cues. An attempt to explain such data purely in terms of transcortical connections must assume that the small ventromedial section of area 18 remaining can function equipotentially for the whole; this seems particularly unlikely when we consider that only short association fibers are found in the visual cortex (**22**), and that the region of area 17 immediately adjacent to the intact remnant of area 18 in Evarts' experiment represents the most peripheral part of the visual field (**76**). One must therefore look elsewhere for pathways to account for extrastriate visual functions.

It seems most likely that transmission via subcortical relay and association nuclei will provide the answer. In support of this, recent physiological studies by Jasper and his coworkers may be cited (**53**). Working with monkeys under local anesthesia and sodium pentobarbital sedation, Jasper, Ajmone-Marsan, and Stoll (**53**) found that local electrical stimulation of area 17 produced strong firing in a narrow zone of the lateral geniculate and simultaneous firing of the adjacent portion of nucleus pulvinaris lateralis. In fact, their results suggest that after-discharge in area 17 is conducted more readily over subcortical pathways than transcortically to the parastriate area. As there are projections from nucleus pulvinaris lateralis to the posterior temporal region, it seems that we have here a means of by-passing the prestriate cortex. Arguing against this view, Chow (**21**) points out that prestriate ablations may cause degeneration in the lateral part of the pulvinar without there being any lasting visual deficit. However, in the one case of this kind reported, the locus of cell degeneration in the pulvinar was not coextensive with the area to which the striate cortex appears to fire (**53**), although admittedly there was some overlap. The evidence at present is not sufficient to settle the question finally one way or the other, but the apparent absence of adequate cortical connections in such cases is presumptive evidence for a subcortical relay of the type described. The fact that combined destruction of posterior-temporal and prestriate cortex causes permanent visual impairment supports this view, suggesting as it does that in such cases both the transcortical and the subcortical pathways from area 17 have been cut.

It remains to discuss briefly the second problem posed by behavioral studies: how destruction of a small area on the ventromedial surface of the temporal lobes, or interruption

of some of the pathways serving it, causes deficits as severe as those seen with extensive damage to the lateral surface of the posterior association cortex. The behavioral evidence clearly shows that the ventromedial portion of the temporal lobe contains an important center for visual elaboration. Physiological studies have revealed an area on the ventral surface to which impulses from the surrounding temporal cortex and from the prestriate area appear to converge, and which itself sends efferent impulses to the prestriate cortex (**87**). Ventromedial-surface extirpations could therefore be expected to interrupt many cortical circuits; it is probable that such lesions also disrupt circuits between temporal lobe and pulvinar. Blum, Chow, and Pribram (**12**) and Chow (**21**) have denied the importance of these subcortical connections, but the evidence does not entirely justify their position.

The principal evidence cited by Blum, Chow, and Pribram is that there is no correlation between visual deficit and either extent or locus of degeneration in the pulvinar. However, absence of retrograde degeneration in a thalamic nucleus is no final proof of lack of connections between that nucleus and the area of cortex destroyed. As Jasper, Ajmone-Marsan, and Stoll (**53**) have pointed out, projecting thalamocortical fibers may have widely distributed collaterals which may protect the cells of origin from degeneration. This would explain the negative results of degeneration studies following moderate-sized ablations in the posterior association cortex. It is known that hemidecortication causes complete degeneration in the pulvinar, while ablations of parietal or temporal cortex alone have only minor effects (**23**, **24**, **106**). A further point is that loss of function may also be caused by interruption of corticofugal connections from temporal lobe to pulvinar. Such corticofugal connections have been suggested by Jasper, Ajmone-Marsan, and Stoll in the study described above.

The main conclusions from this discussion can be summarized as follows. There is a focus for visual elaboration in the ventromedial section of the temporal lobe, and it is reasonable to believe that lesions in this area disrupt important corticocortical and thalamocortical circuits. It has been argued that the deficit seen with extensive lateral surface ablations is partly due to the severing of pathways from area 17 to the temporal lobes; but there is no inconsistency in supposing that the lateral-temporal and prestriate areas are themselves implicated to some extent in the synthesis of visual perceptions.

Review of Human Clinical Studies

In contrast to our present extensive knowledge of the deficits caused by temporal-lobe damage in monkeys, until recently we could only speculate as to the effects of such lesions in man. This ignorance was due in part to the clinical preoccupation with the frontal lobes, in part to a lack of adequate psychological testing when the temporal-lobe case was seen, and perhaps most of all to the unsatisfactory nature of the clinical material. The few psychological studies reported deal with cases of brain tumor, where it is notoriously difficult to assess the full extent of brain damage, and where increased intracranial pressure may produce disturbance far from the site of the primary lesion. Even in cases of penetrating head injury or of vascular accident, it is unlikely that damage will be neatly restricted to the temporal cortex, and,

failing autopsy, we can have only an approximate idea of the area of brain destroyed. The fact that bilateral temporal-lobe lesions in man are rare presents another obstacle to our understanding of temporal-lobe function, although the manifest nonequivalence of the two hemispheres, at least for language, suggests that unilateral studies may be more rewarding at the human than at the animal level.

Despite a lack of precise information, the gross symptomatology of temporal-lobe disease has long been known (**32, 57, 62**). The personality changes which are a conspicuous feature of human temporal-lobe pathology have already been mentioned. Sensory disturbances and complex visual hallucinations are equally striking symptoms and gain in significance when we consider that visual impairment is the characteristic feature of temporal-lobe damage in monkeys. Finally, there is aphasia, which often accompanies lesions of the left temporal lobe, and which will require special discussion later.

Among the sensory disturbances seen in cases of temporal-lobe tumor, auditory anomalies such as tinnitus are the most common (**32**), presumably providing the most reliable localizing sign. The occurrence of olfactory hallucinations is said to imply involvement of the uncus and adjoining rhinencephalic structures at the base of the temporal lobe. Homonymous visual field defects are another frequent symptom and Cushing states that these are usually limited to the upper quadrant contralateral to the lesion and typically show no macular sparing; such defects are taken to indicate damage to the optic radiations as they course round the inferior part of the lateral ventricle into the white matter of the temporal lobe (**26**). In agreement with this, Penfield and Rasmussen (**86**) find that cutting of the white matter below the cortex and five centimeters posterior to the temporal tip produces a homonymous hemianopsia; however, in these cases there appears to be sparing of the contralateral half of macular vision.

As for the more complex visual disturbances, it has been known for some time that various phenomena such as deja vu, micropsia and macropsia, and formed visual hallucinations, may be among the earliest signs of temporal-lobe disease (**32, 42**), and indeed this finding led Hauptmann (**42**) to suggest that the temporal region might mediate important visual functions. Moreover, in epileptic patients paroxysmal discharge within the temporal lobe may give rise to perceptual illusions or hallucinations; such states have been called "dream states" by Hughlings Jackson and "psychical seizures" by Penfield. It is interesting that in such patients electrical stimulation of the exposed temporal cortex may produce complex hallucinations, both auditory and visual, and sometimes appears to reactivate old memories. However, such effects are not elicited by stimulation of the temporal cortex in patients whose epileptic foci are elsewhere (**86**).

It is largely on the basis of these stimulation studies that Penfield has come to attribute to the temporal lobes a unique role in the preservation of memory traces, but the situation is far from simple. Certainly temporal-lobe removal in monkeys does not cause a general memory loss, but has a more specifically visual effect.

It is now time to consider what has been explicitly stated about intellectual functions in temporal-lobe disease. As was suggested earlier, the clinical literature of brain tumor

deals mainly with mental disturbance gross enough to disrupt everyday behavior. Summarizing the findings in such cases, Klebanoff (**58**) states that the presenting symptoms of intellectual deterioration are similar to, but on the whole less severe than, those seen in frontal-lobe tumor; thus the patient may appear confused, and may show memory disturbance and inability to concentrate. The fact that the onset of these nonspecific intellectual defects may be delayed in temporal-lobe cases has led to the view that they may be secondary effects due to involvement of the frontal lobes, an interpretation which clearly reflects the traditional view that the frontal lobes are the site of man's intellectual functions. There is indeed a suggestion that in the temporal-lobe cases the memory loss is apt to be particularly severe, affecting both recent and remote events (**62**), but the fact that removal of the tumor by resection of the temporal lobe normally causes an abatement of the gross symptoms of mental impairment advises caution in drawing such a conclusion.

In two studies where systematic mental testing was done following excision of temporal-lobe tumors, the aim was to study the function of the frontal rather than the temporal lobes. Thus Rylander (**95**) and Halstead (**39**) include temporal-lobe cases in their brain-damaged control groups, and both writers affirm that lesions outside the frontal lobes cause no impairment comparable with that found in their frontal-lobe cases. Yet the value of such assertions may be questioned. In the first place, Rylander deliberately excluded from his control group patients with damage to either the left-parietal or left-temporal lobes, on the grounds that in such cases surgical intervention might greatly injure "the tools for mental activity." This treatment of language as a mere tool is dubious enough, but the risks inherent in such a restricted choice of subjects are here heightened by the use of a test battery composed largely of verbal tests. If, as will be suggested later, the right temporal lobe contributes mainly to the nonverbal aspects of intelligence, then Rylander has not adequately explored the probable areas of intellectual deficit in his seven patients with right temporal-lobe lesions. In Halstead's study, nine temporal-lobe cases are reported, but in only four of them (two left-sided and two right-) did the removals approach lobectomy. Of these, the two left-sided cases showed intellectual impairment at least equal to the average for the frontal-lobe group, while the right-sided cases gave negative results. This suggests that Halstead's special tests are in fact sensitive to left temporal-lobe destruction.

From this point onward the effects of right and left temporal-lobe damage will be treated separately: first, because of the prevalence of language disturbances (aphasia) after lesions of the left temporal lobe; and second, because of the possibility, hitherto unproven, that right temporal-lobe lesions may be more apt than left to cause deficit on certain nonlanguage tests.

Aphasia and the Left Temporal Lobe

Precise information concerning the location of a speech area within the temporal cortex comes from electrical stimulation of the exposed brain in epileptic patients. This technique has revealed an area in the posterior temporal lobe where stimulation may either arrest speech or cause the patient to become aphasic (**86**). The anterior limit of this region is ap-

proximately seven centimeters from the temporal tip, and excision of all cortex anterior to it can safely be made without causing more than a transient aphasia.

The literature of aphasia provides striking evidence that clean surgical destruction is less damaging than an active disease process. Thus Roberts (**93**) points out that the speech area in the posterior temporal cortex can be partially destroyed during the excision of an epileptogenic focus without any permanent aphasia, provided, that is, that the patient's seizures are stopped. Similarly, Fox and German (**31**) and Neilson and Raney (**82**) comment with surprise on the comparative mildness of the residual aphasia seen following complete resection of the left temporal lobe in cases of cerebral tumor, a finding which apparently contrasts sharply with the crippling effect upon language of even minor vascular lesions within the same area (**82**). The data are interpreted by these various authors as showing that the presence of partial damage within a speech area causes greater language disturbance than does the surgical removal of a much larger area. This is a special instance of a principle which has application beyond the field of language disorders: namely, that greater deterioration may result from the presence of abnormally functioning tissue than from mere absence of tissue (**13, 45, 54**).

If we now ask what the special characteristics of temporal-lobe aphasia are, we get no one clear-cut answer. Roberts (**93**) carried out a detailed study of 71 cases of aphasia (following excision of cortex adjoining speech areas in frontal, parietal, and temporal regions), and found no relationship between either the severity or the duration of the language disturbance and the site of the lesion. This conclusion provides support for those who, like Goldstein (**36, 37**), have been consistently opposed to the doctrine that different language functions are differently localized in the left cerebral cortex. However, a more traditional view is that the aphasia seen in posterior lesions is primarily "receptive," (a disturbance of comprehension), as contrasted with the predominantly "expressive" disturbance seen with anterior lesions; Weisenberg and McBride (**108**) explicitly accept this formulation, although they are careful to point out that there is always some expressive defect, making the difference one of degree only. Head (**43**), who lays little stress on receptive disturbances, attributes his syntactic aphasia to lesions of the temporal lobe: a more popular view associates the temporal region with the auditory components of speech, with difficulty both in understanding and in retention of spoken language (**31, 38, 56, 83, 96, 103**). These studies as a whole permit the conclusion that in temporal-lobe aphasia speech comprehension is rather obviously impaired, but not in isolation from other aspects of language (**96, 103**).

We come now to the important question of how far aphasia (and here I include temporal-lobe aphasia) can be regarded as a purely linguistic disturbance which leaves "intelligence" essentially unimpaired, and how far it implies a more general intellectual loss. There are three main schools of thought: according to one (of which Goldstein is the best-known representative), aphasia is merely a manifestation of a more fundamental intellectual disorder (**15, 36, 37, 110, 111**); according to another, there may be loss, which does not affect all aspects of intelligence equally, so that the patient will do very poorly on some tests and comparatively well

on others (**31**, **46**, **108**); or again, there is the third possibility that aphasia is a mere loss of symbols, by which it is implied that the aphasic patient will have difficulties of communication, but no intrinsic difficulty in problem solving (**41**, **50**, **56**, **70**). Those who adopt this last position state that large cortical lesions *also* impair intelligence and that for this very reason such cases are not suitable for studying the typical patterning of abilities in aphasia (**41**, **56**). It will be argued here that it is the second viewpoint for which there is the most empirical support.

It is clear that the testing ground of these rival theories must be the domain of nonverbal skills. Here Kennedy and Wolf (**56**) make the important point that many tests purporting to be nonverbal are in fact not so, since the score depends upon how well the patient has understood the verbal instructions; hence, unless it can be shown in a given instance that the patient has grasped what is required, the work is invalid. This means, in effect, that the only unequivocal results will be those cases in which the patient succeeds on the easy items of a test (showing that he has in fact grasped the instructions), only to fail later on more difficult ones (**46**). Unfortunately, not all tests used in studies of aphasia meet this criterion.

If the results in the clinical literature are taken at their face value, there is still ample proof that aphasia is compatible with normal performance on some nonverbal tests. Meyers (**78**) finds that aphasic patients perform as well as their matched normal controls on nonverbal tests of inductive reasoning of the multiple-choice kind. Similarly, von Kuenburg (**63**) has failed to find clear-cut differences between aphasics and normal control subjects on sorting tests calling for various classifications of colors and objects. Such studies are valuable in underlining the difficulty that many normal persons have on such tasks, a fact which is too often ignored in clinical studies.

Nevertheless it would be idle to deny that aphasic patients often show impairment on nonverbal tests. Van Woerkom (**110**) demonstrated such deficits in individual cases many years ago, and later work has essentially confirmed this finding. As was suggested earlier, the aim here is to show that the deficit is characteristically mild. Weisenberg and McBride (**108**) report that their aphasic patients consistently made better scores on nonverbal tests and on arithmetic than on verbal tests, but that these scores were still distinctly lower than would have been predicted in the case of a normal person with the same educational history. A similar picture emerges from Hebb's (**46**) study of six cases of aphasia following surgical excisions in the dominant hemisphere, and here repeated testing showed that the recovery of verbal and nonverbal functions followed the same time course. Finally, in a report of special interest for this present study, Fox and German (**31**) present follow-up observations in a case of left temporal lobectomy for cerebral tumor: they report "average" performance on a series of nonverbal tests at a time when the patient continued to do poorly on verbal and mathematical tests; at this time (fifteen months after operation), the patient still became confused if he listened to speech for any length of time, although the more overt signs of aphasia had disappeared. These results are not inconsistent with the view that there had been some loss of nonverbal ability, since Fox and German consider that their patient was originally above

average in intelligence, and thus assume that there had been some loss even in nonlanguage fields. It is of course evident that a major difficulty in the way of an accurate appraisal of all such data is the lack of any objective measure of the patients' intellectual status before the onset of aphasia.

It seems that the above studies, incomplete as they are, justify the view that in aphasia the pattern of test performance is likely to be one of severe deficit on verbal tests and relatively mild deficit on nonverbal ones. There is the further point that the degree of this disparity between verbal and nonverbal scores varies markedly from person to person, and with the tests used. These differences, which are very striking, are almost certainly related to the site of the lesion, but how, and to what extent, the data do not permit us to judge. An additional factor may be the degree to which the individual normally relies on verbal cues.

The studies outlined above show the typical patterning of test performance in cases of diffuse pathological damage to the left hemisphere, often involving the temporal cortex. It is reasonable to suppose that a clean surgical removal of the left temporal lobe (without brain tumor) would also affect verbal more than nonverbal performance, but that the over-all deficit would be less severe.

Special Functions of the Right Hemisphere

The absence of aphasia following lesions in the right hemisphere has led to a neglect of possible intellectual loss in such cases, and has probably encouraged the view that the right cerebral cortex plays an essentially ancillary role in intellectual functioning. It will be shown, on the contrary, that the evidence suggests that damage to the posterior association cortex on the right produces deficit different in kind from that caused by comparable damage on the left, rather than an attenuated form of the same disturbance. The data relating specifically to the right temporal lobe are relatively meager, but they are completely consistent with the view just outlined.

The first suggestion that right hemisphere damage might cause its own characteristic deficit comes from Weisenberg and McBride's (**108**) detailed psychometric study of 22 cases of right-sided lesion without aphasia. Although these patients were slightly inferior to matched normal controls in occupational and educational level, this fact cannot account for the irregular nature of the test profiles obtained. Thus, while they approach the normals most closely on some verbal tests, they are significantly inferior on the arithmetic and most of the nonlanguage tests, notably the Porteus Maze Test. The authors conclude that the performance-test battery revealed a significant deficit in the appreciation and manipulation of forms and spatial relationships. These results are quite different from those obtained by the same workers with their aphasic group; the latter, it will be recalled, approached the normal most closely in nonlanguage work and arithmetic. We have no precise knowledge as to the extent of brain damage in the two groups, but Weisenberg and McBride explicitly state that they were ideally matched in this respect. However, it is not clear from the way the data are presented that the absolute mean scores of the right-hemisphere group on nonlanguage tests were lower than those of the aphasic group. If they were not, then we cannot conclude that the right hemisphere is more important than the left for the non-

language tests of Weisenberg and McBride's battery. All that has been shown is that these tests are more sensitive than verbal ones to right-hemisphere damage.

Further evidence on this point is provided by Hebb (**44**), in the case of a patient who had had a complete right temporal lobectomy for the removal of epileptogenic scar tissue. This patient performed well on all verbal tests, with Stanford-Binet Vocabulary at Superior-Adult level and full-scale IQ of 113, but revealed a startling deficit on tests of form perception, both visual and tactual, a finding which is in marked agreement with the results of Weisenberg and McBride. Hebb also reports that his patient had difficulty in following conversations involving more than two or three people, so that he would often make inappropriate remarks. This is an interesting observation, suggesting as it does a type of impairment which neither a conventional test battery nor a clinical interview could be expected to reveal. This study is valuable in that it deals with the effects of a well-localized lesion of one temporal lobe (although it should be added that there had also been a very small cortical excision in the precentral gyrus). Moreover, as the tests were carried out eight years after the temporal lobectomy, it seems safe to assume that the deficits observed were permanent. This study is unfortunately incomplete, in that there are no preoperative data. Yet it is worth noting that McFie, Piercy, and Zangwill (**72**) have described a similar picture of superior verbal intelligence combined with marked loss of "visuo-constructive ability," in the case of a 36-year-old engineer with a glioma of the right temporal lobe. However, they appear to interpret their findings as implying concomitant damage to the parietal lobe. In this instance the patient's profession seems reason enough for regarding the deficit as a direct result of the temporal-lobe tumor.

With the exception of a recent study by the writer (**79**), this exhausts the evidence relating visuo-spatial abilities specifically to the right temporal lobe. There remain a number of studies stressing the importance of the right hemisphere, and more particularly of the right parieto-occipital cortex, for effective spatial organization. The symptoms said to be more common in right- than in left-hemisphere lesions include: disturbance of the coordinates of visual space (**49, 69**); poor performance on visuo-constructive tasks such as map-drawing and block design (**27, 49, 71, 72**); and difficulty in putting on one's clothes, "apraxia for dressing" (**27, 48**). Such findings are of course not in accordance with the traditional view, which states that damage to the right parieto-occipital cortex merely causes neglect of the left side of the body and of the left half of the visual field (**16, 25**). Further, many of the cases on which the above reports are based involve massive, infiltrating tumors, so that bilateral damage can often be suspected. Yet this objection does not dispose of all cases; the growing number of such reports and the diversity of their sources support the idea that there is in fact a significantly higher incidence of visuo-constructive disorders in right-hemisphere lesions than in left.

A Recent Series of Temporal-Lobe Cases

This review may be concluded by summarizing the results of a recent study by the writer (**79**) of 25 cases of temporal-lobe operation by Penfield at the Montreal Neurological Institute. The study included 13 right temporal cases and 12 left, together

with 13 frontal and parietal cases as a control group. The operations were for the relief of focal epilepsy caused by atrophic lesions of the cortex usually dating from birth or early infancy (**85**). All patients were given a battery of sixteen tests before operation and again at the time of their discharge from the hospital about three weeks later, using equivalent forms of the same test wherever possible. The operations were carried out under local anesthesia, and Penfield attempted to reproduce the patient's aura by electrical stimulation of the exposed cortex; the epileptogenic area was then excised. It is important to note that the greatest abnormality was usually found on the ventral and mesial surfaces of the temporal lobe, and that the removal of tissue often extended to include the uncus, amygdaloid nucleus, and hippocampus.

The experimental and control groups were well matched with respect to age, vocabulary, and occupational status, as were also the right and left temporal-lobe groups. The average age for all subjects was 28 years, with a range from 14 to 45. The mean score on Form I of the Stanford-Binet Vocabulary (1916) was 28, which is slightly above the value for normal adults quoted by Weisenberg, Roe, and McBride (**109**). These facts are mentioned to show that we are not dealing with a senile or a mentally retarded group.

Two main results emerged. The first and most consistent was inferiority of the temporal-lobe group on two visual tests (the McGill Picture Anomaly [**47**] and the Wechsler Picture Arrangement), both of which involve complex picture material dealing with everyday social situations. This deficit was present before operation and therefore cannot be attributed to visual field defects. The second finding was the marked inferiority of the right temporal-lobe group on tests of spatial patterning, both visual and tactual. These differences were present before operation, but were intensified after removal of the temporal lobe, a significant further loss being observed in the right temporal group on one test of block design.

That there should be a visual deficit in human temporal-lobe damage is completely consistent with the animal studies reviewed earlier in this paper, the only difference being that in the monkey there must be a bilateral lesion before any visual deficit can be discovered. However, in man, as in monkey, the precise nature of the deficit is not clear. We are justified in concluding that it is specifically visual and not a general impairment of "social intelligence," since no defect was seen on verbal tests, such as the McGill Verbal Situation (**47**), which are also designed to measure some kind of social awareness. On the other hand it is remarkable that no visual tests, other than those mentioned, discriminated between the temporal-lobe patients and the frontal and parietal, although the test battery included the Wechsler Picture Completion, the Benton Visual Memory, the C.M.M. Picture Analogies, and a shortened form of the Raven Progressive Matrices. It seems that the deficit appears when attention has to be given to many aspects of a complex picture, or when different pictures have to be arranged in meaningful order on the basis of slight differences of detail. Visual discriminations of this kind are well practiced in our culture, but they may be difficult to acquire initially. It is clear that much experimental work is needed before we can identify the factors making these tests difficult for patients with temporal-lobe

lesions. The schematic nature of the drawings, the presence or absence of social anomalies, and the mode of presentation of the test may all prove to be important.

The poor performance of the right temporal-lobe group on the Halstead Tactual Formboard (**39**) and on two visual tests of block design provides support for those who contend that the right posterior association cortex is more important than the left for the effective organization of space (**48**, **49**, **71**, **72**).

However, not all patients in the right temporal group showed this spatial disability; the most severe disturbance occurred in cases where the removal extended far back along the inferior surface of the temporal lobe. This may mean either that the posterior temporal region is more important than the anterior for the perception of spatial patterns, or that the important factor is the size of the lesion in the right posterior cortex.

Summary

In both monkey and man temporal-lobe damage can cause emotional and intellectual changes. The present paper has dealt only with the latter.

The main deficit is a visual one. In the monkey, bilateral removal of the temporal lobes severely hampers visual learning and retention, and in man even a unilateral lesion can impair the understanding of complex pictorial material. This loss is not an agnosia in the strict sense, nor is it a simple forgetting of learned discriminations.

Selective ablation studies in the monkey have shown that there is a "focus" on the ventral surface of the temporal lobe, damage to which is almost as detrimental to visual function as is complete lobectomy. Similar effects follow interruption of the transcortical and subcortical connections of this region. In human epileptic patients, the temporal-lobe cases showing pronounced visual deficit are those with focal lesions involving the inferior and mesial aspects of the temporal lobe.

These observations naturally raise the question of the central nervous pathways implicating the temporal cortex in specifically visual functions. It has been argued here that the data do not easily lend themselves to analysis in terms of transcortical systems alone, and that the possible importance of connections between the pulvinar and the temporal cortex should not be overlooked.

Any evaluation of temporal-lobe function in man requires a further breakdown into left and right temporal-lobe lesions. On the left, language disturbances of varying severity commonly follow lesions which interfere with the normal functioning of the speech area in the posterior temporal region, though it is pointed out that a clean surgical removal is apt to produce far milder effects than the continued presence of abnormally functioning tissue. On the right, there may be deficits of space perception, visual and nonvisual. Such deficits have also been found for right parieto-occipital lesions, so that we are justified in regarding the right posterior association cortex as specially important for the perception of spatial patterns.

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Received November 17, 1952.

THE PSYCHOLOGICAL BULLETIN
Vol. 51, No. 1, 1954

A GENERALIZATION OF SIDMAN'S RESULTS ON GROUP AND INDIVIDUAL FUNCTIONS, AND A CRITERION

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In a recent paper Sidman[1] has indicated a devastating criticism of a great deal of current and historical psychological research. He has demonstrated in the particular case of Hull's exponential growth function that if the functional relationship between two variables, x and y, for any particular individual is

$$y = f(x) \qquad [1]$$

then the average y-value, $\bar{y}$, is of the form

$$\bar{y} = g(x) \qquad [2]$$

where $g(x)$ may be a fundamentally different equation from $f(x)$. He shows that if the learning curve for an individual is

$$y = M - Me^{-kx} \qquad [3]$$

then the form of the function based on the average y is not necessarily the same as [3].

Sidman indicates that the kind of analysis he has made can be applied to any function. In this paper we attempt to generalize Sidman's results for any functional relationship, and to provide a criterion for deciding on the legitimacy of the averaging operation as a device for making inferences concerning individual functional relationships.

The major tool that we avail ourselves of is the Maclaurin series:

$$f(x) = f(0) + xf'(0) + \frac{x^2}{2!}f''(0) + \frac{x^3}{3!}f'''(0) + \cdots \qquad [4]$$

where $f(x)$ is our function, $f(0)$ is the value of the function when $x=0$, $f'(0)$ is the value of the first derivative of the function when $x=0$, etc.

Thus, if

$$y = f(a, b, c, \cdots, x) \qquad [5]$$

for a particular individual, then

$$\bar{y} = \frac{\sum y}{n} = \frac{\sum f(a, b, c, \cdots, x)}{n}. \qquad [6]$$

Applying Maclaurin's series, we get

$$y = y_0 + y_0'x + y_0''\frac{x^2}{2!} + y_0'''\frac{x^3}{3!} + \cdots \qquad [7]$$

and

$$\bar{y} = \bar{y}_0 + \bar{y}_0'x + \bar{y}_0''\frac{x^2}{2!} + \bar{y}_0'''\frac{x^3}{3!} + \cdots . \qquad [8]$$

The criterion which we offer to determine the legitimacy of the averaging operation as a basis for making inferences concerning individual functional relationships is as follows: If the coefficients $\bar{y}_0$, $\bar{y}_0'$, $\bar{y}_0''$, etc. are

[1] Sidman, M. A note on functional relations obtained from group data. *Psychol. Bull.*, 1952, **49**, 263–269.

simply functions of the average parameters, $\bar{a}$, $\bar{b}$, $\bar{c}$, etc., then the operation may be considered legitimate. When this criterion is satisfied, the form of the average curve will be identical with the form of each individual curve. It will differ from the individual curves only in the parameters of the function. When the criterion is satisfied the parameter of the group function will be related to the parameter of any individual function in the same way that any mean is related to any individual score. If we restrict ourselves to functions which satisfy the criterion, we may then make inferences concerning the nature of individual functions from the nature of group functions, since the functions will have the same form.

To demonstrate the application of the criterion we will examine two functions

$$y = ax^2 + bx + c \qquad [9]$$

and

$$y = M - Me^{-kx}. \qquad [10]$$

If $y = ax^2 + bx + c$, then

$$\begin{aligned} & & y_0 &= c \\ y' &= 2ax + b & y_0' &= b \\ y'' &= 2a & y_0'' &= 2a. \end{aligned} \qquad [11]$$

Therefore, applying [7] and [8], we find

$$y = c + bx + ax^2 \qquad [12]$$

for the individual, and

$$\bar{y} = \bar{c} + \bar{b}x + \bar{a}x^2 \qquad [13]$$

for the group. Thus, the criterion is satisfied and we can consider the averaging a legitimate operation.

If, however, $y = M - Me^{-kx}$, then

$$\begin{aligned} & & y_0 &= 0 \\ y' &= Mke^{-kx} & y_0' &= Mk \\ y'' &= -Mk^2e^{-kx} & y_0'' &= -Mk^2 \\ y''' &= Mk^3e^{-kx} & y_0''' &= Mk^3 \\ & \vdots & & \vdots \end{aligned} \qquad [14]$$

Applying [7] and [8], we find

$$y = 0 + Mkx - Mk^2\frac{x^2}{2!} + Mk^3\frac{x^3}{3!} - \cdots \qquad [15]$$

and

$$\bar{y} = 0 + \overline{(Mk)}x - \overline{(Mk^2)}\frac{x^2}{2!} + \overline{(Mk^3)}\frac{x^3}{3!} - \cdots. \qquad [16]$$

In [16] the values of the coefficients are not simply functions of the average parameters, $\overline{M}$ and $\bar{k}$, but rather averages of the products, Mk, Mk^2, etc., and therefore the averaging process is not legitimate, as has been indicated by Sidman for this function. It will be apparent that the failure of the exponential growth function to satisfy the criterion results from the fact that $\bar{y}$ is dependent not upon the parameters, as such, but upon a variety of *products* of the parameters; and the mean of a series of products is not necessarily the same as the product of the means. Since the average curve depends upon these individual products, no inferences can be made about the nature of the individual functions from the average curve without knowledge of these individual products. Furthermore, if we knew enough about the individual functions to ascertain these individual products, the average curve would be quite gratuitous.

Received February 13, 1953.

THE PSYCHOLOGICAL BULLETIN
Vol. 51, No. 1, 1954

"THE RESPONSE TO COLOR AND EGO FUNCTIONS": A CRITIQUE IN THE LIGHT OF RECENT EXPERIMENTAL EVIDENCE

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In a recent article in this Journal Fortier (**5**) set out a theory intended to establish a correspondence between color and affect. Beginning from and integrating the theoretical views of Schachtel (**12**) and Rickers-Ovsiankina (**11**), Fortier went on to present a number of publications allegedly supporting the view that response to color is indicative of emotionality. It is the purpose of this note to examine briefly the validity of the evidence brought forward in support of this theory.

The pertinence of the views of Schachtel and Rickers-Ovsiankina need not concern us here. Suffice it to say that they attempt logically to equate color with affect. Instead, it is more important to examine the experimental evidence purporting to throw light on the efficacy of this logic.

Three main lines of experimental evidence are cited: (*a*) evidence from the Rorschach test, (*b*) evidence from the Mosaic Test, and (c) evidence from examination of finger paintings.

In view of the overwhelming majority of studies quoted by Fortier making use of the Rorschach, this paper is confined to an examination of these studies. This is not unreasonable if one remembers that finger paintings and the Mosaic Test are in no way standardized and therefore cannot be regarded as of sufficient validity for the adequate testing of a theory. As Fortier finds no correlation between the results obtained from the Rorschach and those from the Mosaic Test, thus being forced into the construction of a subsidiary *ad hoc* hypothesis, this restriction should act in his favor.

Examination of the evidence presented shows that all the experiments and observations quoted make use of the principle of external validation. Thus, different groups of varying degrees and modes of "emotionality" were used and the relationship between "emotionality" and Rorschach color score indicated. However, in many cases, instead of finding the expected relationship Fortier finds results not in accordance with his views. Thus, when he finds that Rorschach investigations do not support Burt's (**2**) view on the emotionality of delinquents, Fortier discards this view for another *which is based upon Rorschach findings*, and so finds an impressive correspondence between the new view of the affective state of the delinquent and his color score on the Rorschach. Similarly, when he finds that Rorschach findings do not fit in with the traditional view, he discards the orthodox view of the emotional sterility of the schizophrenic in favor of Beck's (**1**) opinion that those patients do have emotional experiences. All this despite the fact that Beck's opinion is largely derived from his experience with the Rorschach test. There are other instances of similar *ad hoc* reasoning, but to continue would be pointless.

Even with the genetic studies Fortier finds it difficult to prove his point and complains that little evidence is available. But were Fortier acquainted with the works of Lindberg (**9**), Eysenck (**4**), and others he would find numerous papers quoted demonstrating the decrease of color reaction with increasing age in children, evidence which would enable him to accept the general view of the increase of emotional control with age. This evidence comes from experiments with tests of color-form attitude, a line of approach which Fortier seems unaware of, for there is no mention of it in his text, although one reference does appear in the bibliography (Oeser [**10**]). However, no studies using the external criterion of "emotionality" are of much use in attempting to find a personality correlate of color reactivity, for emotionality itself is but a vague and ill-defined term.

If one wishes to discover whether or not reaction to color in general does have some correlate in the field of personality, then some indirect approach must be used. One such approach would be that of internal validation. This involves administering a number of tests involving color as a scorable item and correlating the resultant test scores. Unless significant correlations were obtained between such tests, color reactivity could not be regarded as having any personality correlates, for if a particular personality attribute were associated with high reaction to color then this reaction would have to occur on all tests involving color reaction as a scorable response.

Examination of the data of Eysenck (**4**), Clarke (**3**), and Lindberg (**9**) (reworked) shows an average correlation between color-form tests of the order of .2. This leads the first two of these authors to conclude that, except in the eventuality of isolating a factor of color-form attitudes, reactions to color-form tests are relatively specific to each test. Lindberg reached the opposite conclusion through a false statistical analysis, but the reanalysis of his data as shown in Keehn's study (**8**) supports the opinions of Eysenck and Clarke.

However, a factor analysis of the intercorrelation matrix obtained from a number of color-form tests given to a large group of subjects did result in the extraction of a color-form factor (**8**). Evidence presented from the same analysis (**7**) showed that the conventional Rorschach color score had no saturation on this factor but, instead, had a loading on a factor of whole-part attitude.[1] Thus the results from this study were interpreted as an indication that color was used as a determinant in the Rorschach only if the subject attempted to integrate as much of the stimulus as possible into his response. This shows the traditional view of the role of color in the Rorschach test to be incorrect.

Support for the need of some reinterpretation of the part played by color in this test comes from an analysis of the experiments carried out to determine the validity of the concept of color shock. This concept is also relevant to the color-emotionality hypothesis, being, in effect, an extension of it paralleling the constriction of emotion with the suppression of color responses. In a review of a number of studies with color shock (**6**) hardly any evidence for the dependence of color shock signs upon color was found.

Thus, and this is the crux of the

[1] This evidence, of course, was not available to Fortier during the preparation of his paper.

matter, inasmuch as the Rorschach color responses are at best only indirectly affected by color, the use of the results of Rorschach studies either to support or to refute the color-emotionality hypothesis is invalid. Hence the great majority of the evidence put forward by Fortier in support of his theory must be regarded as irrelevant.

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Received March 31, 1953.

The Psychological Bulletin
Vol. 51, No. 1, 1954

AN APPRAISAL OF KEEHN'S CRITIQUE OF "THE RESPONSE TO COLOR AND EGO FUNCTIONS"

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There are essentially three main criticisms levied by Keehn:

1. *Criticisms based upon Keehn's recently completed work.* It becomes difficult to formulate a reply to criticisms derived from original work which has not yet been published. What can be said here is based only upon the brief excerpt which Keehn included in his critique. Keehn states: " . . . inasmuch as the Rorschach color responses are at best only indirectly affected by color the use of Rorschach studies either to support or to refute the color-emotionality hypothesis is invalid." The path which he follows from his studies to this conclusion is rather difficult to follow, particularly as it applies to the writer's theoretically oriented review.

Keehn does make two direct state-

[1] Now at the VA Hospital, Northampton, Mass.

ments in his critique before arriving at the rather sweeping conclusion quoted above: (*a*) "This [referring to his color-form factor analysis] shows the traditional view of the role of color in the Rorschach test to be incorrect." (*b*) " . . . hardly any evidence for the dependence of color shock signs upon color was found."

In regard to the first statement, the writer is largely in agreement with Keehn that the traditional view of the role of color in the Rorschach test is in many respects incorrect. Such was the point and purport of his entire paper. However, an evaluation of this statement by Keehn could have been better made if Keehn had stated its rationale more specifically. In regard to Keehn's findings of color shock and its relevance to the role of color, it can be seen by a scrutiny of the writer's paper (**2**) that in no whit does the theory he presents depend upon any interpretation of color shock.

If the two statements quoted here are the vehicles which carried Keehn to his final conclusion, it can be seen that his conclusion has no relevance to the writer's paper.

Keehn, upon receipt of my appraisal of his critique, very kindly sent me a slightly revised copy of the critique, in which he stresses somewhat more the interpretation of his color-form factor analysis. This revision, in turn, prompted me into dealing more directly with these interpretations. They are as follows:

a. Reactions to color-form tests are relatively specific to each test.

b. "Thus, the results from this study were interpreted as an indication that color was used as a determinant in the Rorschach only if the subject attempted to integrate as much of the stimulus as possible into his response."

In regard to *a*, the writer wishes to point to what Keehn has called this writer's "subsidiary *ad hoc* hypothesis" concerning the Mosaic Test. The writer, in that formulation—whatever it may later be called—stressed that the specific response to, or mode of usage of, color is dependent upon the inherent dynamics of the Mosaic Test—or the functions which the individual had to perform in executing the test. Color was still related to affect in the manner in which it was sketched in the theory. The fairly strong implication was that the specific responses elicited by one test might be different from responses elicited by another test simply because the operations satisfying the requirements of the two tests might be different.

In regard to *b*, if affects can be regarded as part of the totality of any situation, that individual who is regarded as more capable of handling affects is the one who can best integrate affects into the totality of the situation, and can control them.

2. *Failure to pursue the line of attack developed by Lindberg and others.* The original bibliography compiled by the writer consisted of several hundred articles. Limitations of space prevented a meaningful discussion of some of these studies. Other studies were ambiguous or otherwise unclear. In an original draft of his review paper, the writer included the particular study by Lindberg (**3**) which Keehn cited. However, he found that a great many of the interpretations and conclusions made by Lindberg were completely immersed in a system of psychology which had not appeared to any great extent in the American literature. He felt that a fair treatment of Lindberg's interpretations and conclusions could not be made without dealing at

great length with the particular psychological system used by Lindberg. Because space was limited, such an elaboration was impossible, and Lindberg's study was dropped.

3. *The utilization of* ad hoc *hypotheses.* It was the writer's intent "to present a theory . . . and second, to examine a number of studies involving color in an effort to substantiate and clarify the theory" (**2**, p. 41). Although this theory has been in existence for a number of years, it is by no means in common usage. Following this presentation, the writer reviewed a number of studies to test the theory. In its strict sense, the term "test" would have implied in the situation under discussion a mere listing of studies with the words "yes" and "no" indicating whether the study supported the theory or refuted it. And following such a listing, one could perhaps have taken a "vote" by counting the "yeses" and "noes." The writer felt that much more could be gained by subjecting the data to a logical analysis, particularly when interpretations and conclusions from the data were contradictory to prevalent points of view.[2] In other words, particularly in those cases where interpretations and conclusions from data were contrary to prevalent views, the writer made reinterpretations of the original data, stated the reinterpretations, and subsequently attempted to examine their implications. The reinterpretations and inferences derived therefrom thus become "*ad hoc*" in the sense that these formulations were not made until the studies were seen, and their conclusions suggested the need for reinterpretation. Benjamin has this to say concerning *ad hoc* constructs or explanations: "Explanation consists in the transfer of properties and relations from the explanatory entity to that which is to be explained; in explanation one increases the content of the data by adding to them the features of the hypothesis. But if a construct has been obtained by direct derivation from the data, there is clearly no possibility of turning about and explaining the data in terms of the construct. The construct does not contain that increment of novelty without which explanation is impossible. A pure construct used as an explanatory entity produces an *ad hoc* or *verbal* explanation" (1, p. 186). The writer drew his reinterpretations and inferences in many cases from areas which lay outside the specific data reported in the studies which he reviewed. Thus, in the sense in which the term is used by Benjamin, the reinterpretations are not *ad hoc.* The value of Keehn's criticism would have been enhanced had he specified the sense in which he felt these reinterpretations to be *ad hoc.*

[2] It is the writer's opinion that his adaptation of Schachtel's and Rickers-Ovsiankina's theories is not among the prevalent points of view, and he so stated in his paper.

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Received May 15, 1953.

THE PSYCHOLOGICAL BULLETIN
Vol. 51, No. 1, 1954

CURRENT INTERPRETATION AND SIGNIFICANCE OF LLOYD MORGAN'S CANON

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In 1932 Nagge (**14**) showed that the meaning of Lloyd Morgan's Canon had been greatly transformed by later psychologists so that, permitting a variety of interpretations, it was difficult to apply and needed clarification. In some instances more recently, as in the earlier paper of Adams (**1**), the Canon has been interpreted as a version of the law of parsimony (Boring, **2**, pp. 474, 498, 623, 629; Griffith, **4**, p. 322; Harriman, **5**, p. 255; Hilgard, **6**, p. 343; Munn, **12**, p. 2; **13**, p. 2; Skinner, **17**, p. 4; Warden, Jenkins, and Warner, **20**, p. 26; Waters, **22**, p. 24; Woodworth, **25**, pp. 49 f.). In several of these (**2**, pp. 563, 623; **5**, **12**, **13**, **25**) and others (**18**, p. 250; **24**, pp. 124, 747), the Canon has been explicitly interpreted as a doctrine of simplicity. It has been related to Occam's Razor (**2**, p. 498; **4**, **5**, **25**), has had imputed to it an antimentalism (**4**, **5**, **17**), or what amounts to a behavioristic reductionism (**17**), but also a futile subjectivism (**20**, pp. 45–54). The Canon has been criticized on several of these bases. This variety of interpretations poses a problem in both accuracy and the historical continuity of thought. What *did* Morgan say?

The Canon and its context. Lloyd Morgan was mainly concerned with the continuity of mental evolution. His Canon was a methodological rule for placing animals in the scale of development. The first statement (1894) was: "In no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale" (**7**, p. 53). In a revised edition Morgan added to this a clarifying restatement: "In no case is an animal activity to be interpreted in terms of higher psychological processes, if it can be fairly interpreted in terms of processes which stand lower in the scale of psychological evolution and development" (**8**, p. 59). In 1925 he extended the range of the Canon to embrace his now explicit emergent evolution: "In no instance should we interpret events in terms of concepts appropriate to a higher level of emergence if they can adequately be interpreted in terms of concepts appropriate to a lower level of emergence" (**10**, p. 61). And in his American autobiography (1932) he speaks of "the evolutionary canon that we should not interpret an earlier and lower stage of mental development in terms applicable only to the interpretation of the higher and later stage" (**11**, p. 262; cf. also pp. 260 ff.).

The original Canon was set in a context of double-aspect monism. Introspection was necessary for direct acquaintance with psychical processes. Double inductions were required for inferring mental processes in other organisms, either human or animal, in which "inductions reached through the objective study of certain physical manifestations have to be interpreted in terms of in-

ductions reached through the introspective study of mental processes" (**7**, p. 47, cf. pp. 36–53).

The context was also one of an evolutionary naturalism which recognized the appearance of novel functions in a process called "selective synthesis" (**7**, ch. 19). "Synthesis, with new properties at critical turning-points, was the burden of my evolutionary contention," Morgan later commented, changing the term to "emergence" (**9**, p. 302 ff.).

While Morgan recognized that a given process might be more highly developed in one animal type than in another, he was not willing to accept a reductionism whereby "all forms of animal life from the amoeba upwards have all the faculties of man," or that "in the higher forms of life the introduction of the higher faculties has been effected by some means other than that of natural evolution" (**7**, p. 58). He argued that "any animal may be at a stage where certain higher faculties have not yet been evolved from their lower precursors; and hence we are logically bound not to assume the existence of these higher faculties until good reasons shall have been shown for such existence" (p. 59). Thus the Canon, in Morgan's conception, was one of the devices displacing an uncritical assumption of identity between the human and animal mind.

The context of the Canon shows that Morgan was not trying to explain the behavior of a single animal in a single experimental situation, but to determine what characteristics of consciousness could be attributed to a given animal type. In his later clarification, "lest the range of the principle be misunderstood," Morgan carefully adds "that the canon by no means excludes the interpretation of a particular activity in terms of the higher processes, if we already have independent evidence of the occurrence of these higher processes in the animal under observation" (**8**, p. 59). Currently Hilgard makes a similar point, but erroneously gives it as an *inversion* of the Canon. "As now inverted," he writes, "it might be paraphrased: 'An organism capable of ideational problem-solving may also use ideas in learning situations in which they would be theoretically unnecessary" (**6**, p. 345).

Antimentalism and reductionism. Despite its introspective basis, the Canon has been interpreted as eliminating conscious processes through an antimentalism or by implying in it a reductionism. Harriman quotes Occam's Razor—"entities must not be multiplied beyond necessity"—and states that "Morgan accepted this view, indicating that anecdotes, attribution of human mental activities to animals, and projection of introspections have no place in animal psychology" (**5**, p. 255). Griffith makes the Canon, interpreted on the basis of Occam's Razor, appear to emphasize physical factors and not to necessitate the inclusion of mental ones (**4**, p. 322). Skinner writes: "Darwin, insisting upon the continuity of mind, attributed mental faculties to subhuman species. Lloyd Morgan, with his law of parsimony, dispensed with them in a reasonably successful attempt to account for characteristic animal behavior without them" (**17**, p. 4). And Munn is not clear (**13**, pp. 1 f.).

Waters with some approval says that "rigid application of Morgan's Canon" is criticized by Gestalt psychologists on the ground of need for "qualitative descriptions that are not allowed on the basis of this canon" (**22**, pp. 24 f.). Yet the Canon was consistent with the concept of emer-

gent novelties and the configurational qualities such as depth perception in which "the out-thereness" results from sensations coalescing "into a synthesis which has a new and determinate character" (**7**, p. 352; cf. also ch. 1). The criticism seems to be of a misinterpretation rather than of a rigid application of the Canon.

Waters (**21**) attacks an anti-anthropomorphism which he ascribed ambiguously either to the Canon or to later interpretations of it. What Waters in general seems to find necessary is a critical anthropomorphism in the sense of introspectively oriented concepts. Since this was the actual basis for Morgan's Canon, the value of historical continuity of thought is lost in Waters' treatment, all the more emphatically because he designates Tolman, Krechevsky, and Maier as anthropomorphic in this sense (pp. 535 f.). Similarly, Morgan's current significance is lost when Hilgard follows a historical treatment by his own criticism of the requirement that explanations be only in palpable terms (**6**, pp. 343 f.). And the humanistic, textbook aims of Stagner and Karwoski are hardly realized by stating that Morgan "attacked 'anthropomorphism' (interpreting animal behavior in human terms) with his famous canon" (**18**, p. 250). The Canon is a device for selecting in interpretation introspectively derived processes, not for completely eliminating them, which would be to eliminate use of the Canon (cf. **11**, p. 261).

Occam's Razor and parsimony. The antimentalistic contradiction of applying Occam's Razor through the Canon was noted above in Griffith and Harriman. In the sense of Harriman's literal translation (*supra*) of the classical Latin statement (**19**), Occam's Razor is applied when we adhere to a *paucity* of assumptions, whereas Morgan's Canon refers to *lower* processes of development. The total number of scientific entities is not decreased by the Canon in so far as they are introspectively available, but the number of emergent functions attributed to an animal of a given type may be decreased, so that for this animal the net effect is in line with Occam's Razor, while the admonition in the Canon regards the lowness of the functions, not their paucity.

As an application of the law of parsimony and of Occam's Razor, Boring argues that however useful the Canon may have been in counteracting extremes of Romanes' anthropomorphism, times and problems have changed, and entities of necessity must now be increased (**2**, pp. 474, 498, 563). Since the Canon allows higher processes when independent evidence makes their assumption necessary, one may reasonably ask, why not further decrease probability of error by utilizing knowledge of developmental sequences?

While Occam's Razor and the law of parsimony have no doubt sometimes been identified in science, the latter especially is a loose term subject to a variety of meanings, some of which go as far back as Aristotle (**2**, pp. 498 f.; **3**, p. 231; **14**; **15**, p. 472; **16**, pp. 114 f.; **19**; **23**, p. 155). It is only confusing to say, without adequate qualification, that Morgan's Canon is derived from either, and of doubtful accuracy to identify it with them. The confusion can be compounded by the doctrine of simplicity.

Simplicity of explanation. Woodworth criticizes Morgan's Canon as a doctrine of simplicity, raising the question of what is simpler. Animal

psychologists have usually regarded movement as simpler than perception, he says, but "judging by ourselves we should say that nothing is easier than seeing an object and that no learned reaction is simpler than recognition" (**24**, p. 124).

Such a criticism was anticipated by Morgan himself (**7**, p. 54). He answered, first, that "surely the simplicity of an explanation is no necessary criterion of its truth," and in many cases "the simplest explanation is not the one accepted by science" (pp. 54 f.). Second, "the simplicity of the explanation of the phenomena of animal activity as the result of intellectual processes, can only be adopted on the assumption of a correlative complexity in the mental nature of the animal as agent" (p. 55). This assumption had to be justified by induction. Thus Morgan distinguished between the doctrine of simplicity and his Canon, which makes no reference to it.

Morgan did suppose that in evolution the earlier physical and mental processes were simpler (p. 8). He postulated an evolutionary development with an increasing complexity of organic structure, of correlated activities, and of mental and psychic functions (p. 55). This made the problem of psychic level which logically justified the Canon. But in this development appeared new properties. Indeed, years later Morgan turned his 1925 version of the Canon on the radical behaviorist by saying that "we should not interpret the lower forms of trial and error as affording evidence of trial and error of the kind that characterises reflective procedure" (**10**, p. 61). Thus a doctrine of higher and lower in the level of development can be consistent with the presumptive fact of evolutionary development from simplicity to complexity without the methodological rule being one of simplicity at all. Failure to recognize this possibility seems to explain a good deal of the misinterpretation of the Canon.

Comment. Aside from their historical inaccuracies, many current misinterpretations of Morgan's Canon have *sui generis* failed to take advantage of possible logical developments. Without contending that Morgan's methodology represents the last word, one can recognize in it some of the essentials for integrating modern introspective and comparative psychology. Whether this gain through historical continuity can be realized depends upon an accurate and significant interpretation of that methodology, including the Canon.

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Received May 25, 1953.

THE PSYCHOLOGICAL BULLETIN
Vol. 51, No. 1, 1954

PSYCHOLOGICAL NECROLOGY (1928–1952)

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It is not easy to find the dates of death of prominent men because the who's-who compendia usually omit the sketch when the man dies and thus never publish the date. For this reason one of us published in 1928 the dates of death of 248 prominent psychologists who died during the twenty-five year period 1903–1927 (1). Now, a quarter of a century later, we undertake for the same reason to present a similar list for the next twenty-five years, 1928–1952. How did we get the names and data?

We searched for this period *Psychological Abstracts*, the *Directories of the American Psychological Association*, *American Men of Science*, *Science*, and *Archives de Psychologie*, which was especially rich in European notices during Claparède's incumbency. We added, of course (and continued later to add), various prominent names which came incidentally to our attention, and we came out with a list of 809 deceased psychologists, one death every twelve days for a quarter century! Since not all these persons were important enough to list in crowded journal space, we had to select.

So we gave the list of 809 to L. M. Terman, H. S. Langfeld, Wayne Dennis, J. G. Beebe-Center, E. B. Newman, and the senior author of this paper, asking them to indicate which names they did not recognize at all, which names they recognized without further association, which names they recognized and knew something about, as well as the names they recognized but thought too unimportant for inclusion in the final list and the names they recognized but thought deviated too far in the sociotropic, biotropic, or physicotropic direction to count as psychologists. Then we edited these results, making decisions.

We omitted such prominent names as Ruth Benedict, Bergson, Franz Boas, C. B. Davenport, Hrdlicka, Korzybski, Malinowski, Raymond Pearl, and J. J. Thomson, persons who are or were important to psychologists but not, in our opinion, quite psychologists. We allowed ourselves, as our list shows, to deviate more freely in the direction of physiology and neurology, in accordance with psychology's tradition in which *physiologische Psychologie* was more important than *Völkerpsychologie*. So we include Cajal, H. H. Donaldson, Ferrier, Flechsig, W. M. Wheeler, and some other biotropes. We let in sociotropes as remote as Havelock Ellis, and the important psychoanalysts, like Adler, Ferenczi, and Rank. Freud, of course. Nor could we quite bring ourselves to exclude Karl Pearson.

Next we had regard to the votes of our jury of six. We accepted for the list every name, not already excluded as peripheral to psychology, that received six, five, or four votes as being known about in addition to being barely recognized. That gave us nearly all of our list. Finally we accepted 12 names with only three votes, mostly names where once famous psychologists were known to

the older members of the jury and not to the younger. The senior author then translated his prejudices into authority and took in three persons with only two votes and Beatrice Edgell whose contribution to psychology was recognized by none of the jury other than himself. Nobody was included on bare recognition alone. Thus we obtained a list of 193 persons, to which we have added gratuitously entries for three important psychologists who died early in 1953: Kafka, Katz, and Marbe.

The list is published for the purpose of recording exact dates of death. Instead of giving age at time of death we have included in all cases exact date of birth. It has not been easy to obtain these data. We have consulted over two dozen files, from the *New York Times* and *Who Was Who in America* to the published necrologies in the standard scientific journals. For the last score of entries we wrote letters to persons who might know about the deceased or have ready access to local records. Professor P. Fraisse of Paris and Professor H. v. Bracken of Braunschweig have been particularly helpful in obtaining some of the French and German items.

Our geographical entry is meant for identification. It is not the place of death, which is sometimes an obscure locality to which an old man had retired. We have tried to give what seemed to us to be the place of last important professional activity.

Here is the list.

Ach, Narziss Kasper, Göttingen, b. 7 June 1848, d. 25 July 1946.

Adler, Alfred, Vienna and New York City, b. 7 Feb. 1870, d. 28 May 1937.

Adler, Herman Morris, Univer. California, b. 10 Oct. 1876, d. 6 Dec. 1935.

Angell, Frank, Stanford Univer., b. 8 July 1857, d. 2 Nov. 1939.

Angell, James Rowland, Yale Univer., b. 8 May 1869, d. 4 Mar. 1949.

Angier, Roswell Parker, Yale Univer., b. 21 Oct. 1874, d. 24 June 1946.

Arps, George Frederick, Ohio State Univer., b. 23 Jan. 1874, d. 16 Sept. 1939.

Aveling, Francis, London, b. 25 Dec. 1875, d. 6 Mar. 1941.

Babcock, Harriet, New York City, b. 7 Jan. 1877, d. 17 Dec. 1952.

Bagley, William Chandler, Columbia Univer., b. 15 Mar. 1874, d. 1 July 1946.

Baldwin, Bird Thomas, Univer. Iowa, b. 31 May 1875, d. 12 May 1928.

Baldwin, James Mark, Paris, b. 12 Jan. 1861, d. 8 Nov. 1934.

Becher, Erich, Munich, b. 1 Sept. 1882, d. 5 Jan. 1929.

Bingham, Walter Van Dyke, Washington, D. C., b. 29 Oct. 1880, d. 8 July 1952.

Bleuler, Paul Eugen, Zürich, b. 30 Apr. 1857, d. 15 July 1939.

Bolton, Thaddeus Lincoln, Temple Univer., b. 27 July 1865, d. 3 Jan. 1948.

Book, William Frederick, Indiana Univer., b. 10 June 1873, d. 22 May 1940.

Bourdon, Benjamin Bienaimé, Rennes, b. 5 Aug. 1860, d. 11 July 1943.

Breese, Burtis Burr, Univer. Cincinnati, b. 17 May 1867, d. 31 July 1939.

Brett, George Sidney, Univer. Toronto, b. 5 Aug. 1879, d. 27 Oct. 1944.

Brigham, Carl Campbell, Princeton Univer., b. 4 May 1890, d. 24 Jan. 1943.

Brill, Abraham Arden, Columbia Univer., b. 12 Oct. 1874, d. 2 Mar. 1948.

Buchner, Edward Franklin, Johns Hopkins Univer., b. 3 Sept. 1868, d. 22 Aug. 1929.

Burks, Barbara Stoddard, Columbia Univer., b. 22 Dec. 1902, d. 25 May 1943.

Burnham, William Henry, Clark Univer., b. 3 Dec. 1855, d. 25 June 1941.

Burrow, Trigant, Lifwynn Found., b. 7 Sept. 1875, d. 24 May 1950.

Cajal, Santiago Ramón y, Madrid, b. 1 May 1852, d. 17 Oct. 1934.

Calkins, Mary Whiton, Wellesley Coll., b. 30 Mar. 1863, d. 26 Feb. 1930.

Cannon, Walter Bradford, Harvard Univer., b. 19 Oct. 1871, d. 1 Oct. 1945.

Cason, Hulsey, Univer. Miami, b. 21 Feb. 1893, d. 30 Apr. 1951.

Cattell, James McKeen, New York City, b. 25 May 1860, d. 20 Jan. 1944.

Claparède, Edouard, Geneva, b. 24 Mar. 1873, d. 29 Sept. 1940.

Coghill, George Ellett, Wistar Inst., b. 17 Mar. 1872, d. 23 July 1941.

Conklin, Edmund Smith, Indiana Univer., b. 19 Apr. 1884, d. 6 Oct. 1942.

Coover, John Edgar, Stanford Univer., b. 16 Mar. 1872, d. 19 Feb. 1938.

Dearborn, George Van Ness, New York City, b. 15 Aug. 1869, d. 12 Dec. 1938.

Decroly, Ovide Jean, Brussels, b. 23 July 1871, d. 12 Sept. 1932.

Delabarre, Edmund Burke, Brown Univer., b. 25 Sept. 1863, d. 16 Mar. 1945.

Delacroix, Henri, Paris, b. 2 Dec. 1873, d. 3 Dec. 1937.

De Sanctis, Sante, Rome, b. 7 Feb. 1862, d. 20 Feb. 1935.

Dessoir, Max, Berlin, b. 8 Feb. 1867, d. 19 July 1947.

Dewey, John, Columbia Univer., b. 20 Oct. 1859, d. 1 June 1952.

Dockeray, Floyd Carlton, Ohio State Univer., b. 15 May 1880, d. 15 Jan. 1949.

Dodge, Raymond, Yale Univer., b. 20 Feb. 1871, d. 8 Apr. 1942.

Donaldson, Henry Herbert, Wistar Inst., b. 12 May 1857, d. 23 Jan. 1938.

Downey, June Etta, Univer. Wyoming, b. 13 July 1875, d. 11 Oct. 1932.

Drever, James, Edinburgh, b. 8 Apr. 1873, d. 11 Aug. 1950.

Dumas, Georges, Paris, b. 6 Mar. 1866, d. 13 Feb. 1946.

Duncker, Karl, Swarthmore Coll., b. 2 Feb. 1903, d. 23 Feb. 1940

Dunlap, Knight, Univer. California Los Angeles, b. 21 Nov. 1875, d. 14 Aug. 1949.

Dusser de Barenne, Johannes Gregorius, Yale Univer., b. 6 June 1885, d. 9 June 1940.

Edgell, Beatrice, London, b. 26 Oct. 1871, d. 10 Aug. 1948.

Ehrenfels, Christian von, Prague, b. 20 June 1859, d. 8 Sept. 1932.

Ellis, Henry Havelock, London, b. 2 Feb. 1859, d. 8 July 1939.

Farrand, Livingston, Cornell Univer., b. 14 June 1867, d. 8 Nov. 1939.

Ferenczi, Sándor, Budapest, b. 7 July 1873, d. 22 May 1933.

Fernald, Grace Maxwell, Univer. California Los Angeles, b. 29 Nov. 1879, d. 16 Jan. 1950.

Fernald, Mabel Ruth, Cincinnati, Ohio, b. 7 May 1883, d. 9 Oct. 1952.

Ferree, Clarence Errol, Johns Hopkins Univer., b. 11 Mar. 1877, d. 26 July 1942.

Ferrier, David, London, b. 13 Jan. 1843, d. 19 Mar. 1928.

Flechsig, Paul Emil, Leipzig, b. 29 June 1847, d. 22 July 1929.

Fletcher, John Madison, Tulane Uni-

ver., b. 27 June 1873, d. 12 Dec. 1944.

Forel, August, Zürich, b. 1 Sept. 1848, d. 27 July 1931.

Franz, Shepherd Ivory, Univer. California Los Angeles, b. 27 May 1874, d. 14 Oct. 1933.

Freud, Sigmund, Vienna and London, b. 6 May 1856, d. 23 Sept. 1939.

Frey, Max von, Würzburg, b. 16 Nov. 1852, d. 25 Jan. 1932.

Fröbes, Joseph, Rome, b. 26 Aug. 1866, d. 24 Mar. 1947.

Gamble, Eleanor Acheson McCulloch, Wellesley Coll., b. 2 Mar. 1868, d. 30 Aug. 1933.

Garth, Thomas Russell, Univer. Denver, b. 24 Dec. 1872, d. 20 Apr. 1939.

Geissler, Ludwig Reinhold, Randolph-Macon Women's Coll., b. 22 Sept. 1879, d. 15 Dec. 1932.

Gelb, Adhémar, Frankfurt a. M., b. 18 Nov. 1887, d. 7 Aug. 1936.

Gilliland, Adam Raymond, Northwestern Univer., b. 5 Oct. 1887, d. 1 Dec. 1952.

Goldscheider, Alfred, Berlin, b. 4 Aug. 1858, d. 10 Apr. 1935.

Gosset, William Sealy ("Student"), London, b. 13 June 1876, d. 16 Oct. 1937.

Groos, Karl, Tübingen, b. 10 Dec. 1861, d. 3 Apr. 1946.

Grünbaum, Anton Abram, Utrecht, b. 23 May 1885, d. 10 Jan. 1932.

Haggerty, Melvin Everett, Univer. Minnesota, b. 17 Jan. 1875, d. 6 Oct. 1937.

Haines, Thomas Harvey, Montclair, N. J., b. 4 Nov. 1871, d. 2 Mar. 1951.

Hamilton, Gilbert Van Tassel, Santa Barbara, Calif., b. 15 Jan. 1877, d. 16 Dec. 1943.

Head, Henry, London, b. 4 Aug. 1861, d. 8 Oct. 1940.

Hecht, Selig, Columbia Univer., b. 8 Feb. 1892, d. 18 Sept. 1947.

Henmon, Vivian Allen Charles, Univer. Wisconsin, b. 27 Nov. 1877, d. 10 Jan. 1950.

Heymans, Gerardus, Groningen, b. 17 Apr. 1857, d. 18 Feb. 1930.

Hobhouse, Leonard Trelawney, London, b. 8 Sept. 1864, d. 21 June 1929.

Höffding, Harald, Copenhagen, b. 11 Mar. 1843, d. 2 July 1931.

Hollingworth, Leta Stetter, Columbia Univer., b. 25 May 1886, d. 27 Nov. 1939.

Holt, Edwin Bissell, Princeton Univer., b. 21 Aug. 1873, d. 25 Jan. 1946.

Holway, Alfred Harold, Eastman Kodak Co., b. 28 Feb. 1905, d. 1 June 1948.

Hornbostel, Erich von, Berlin and Cambridge, Eng., b. 25 Feb. 1877, d. 13 June 1935.

Horney, Karen, New York City, b. 16 Sept. 1885, d. 4 Dec. 1952.

Hull, Clark Leonard, Yale Univer., b. 24 May 1884, d. 10 May 1952.

Husserl, Edmund, Freiburg i. Br., b. 8 Apr. 1859, d. 27 Apr. 1938.

Isaacs, Susan Sutherland, London, b. 24 May 1885, d. 12 Oct. 1948.

Jaensch, Erich Rudolf, Marburg, b. 26 Feb. 1883, d. 1 Feb. 1940.

Janet, Pierre, Paris, b. 30 May 1859, d. 24 Feb. 1947.

Jastrow, Joseph, Univer. Wisconsin, b. 30 Jan. 1863, d. 8 Jan. 1944.

Jelliffe, Smith Ely, New York City, b. 27 Oct. 1866, d. 25 Sept. 1945.

Jenkins, John Gamewell, Univer. Maryland, b. 30 May 1901, d. 30 Jan. 1948.

Jennings, Herbert Spencer, Johns Hopkins Univer., b. 8 Apr. 1868, d. 14 Apr. 1947.

Judd, Charles Hubbard, Univer. Chicago, b. 20 Feb. 1873, d. 18 July 1946.

Kafka, Gustav, Würzburg, b. 23 July 1883, d. 12 Feb. 1953.

Katz, David, Stockholm, b. 1 Oct. 1884, d. 2 Feb. 1953.

Kellogg, Chester Elijah, McGill Univer., b. 11 Nov. 1888, d. 9 July 1948.

Kiesow, Federico, Turin, b. 28 Mar. 1858, d. 9 Dec. 1940.

Kirkpatrick, Edwin Asbury, Fitchburg St. Teach. Coll., b. 29 Sept. 1862, d. 4 Jan. 1937.

Kirschmann, August, Leipzig, b. 21 July 1860, d. 24 Oct. 1932.

Klemm, Gustav Otto, Leipzig, b. 8 Mar. 1884, d. 5 Jan. 1939.

Koffka, Kurt, Smith Coll., b. 18 Mar. 1886, d. 22 Nov. 1941.

Kries, Johannes von, Freiburg i. Br., b. 6 Oct. 1853, d. 30 Dec. 1928.

Kuhlmann, Frederick, St. Paul, Minn., b. 20 Mar. 1876, d. 19 Apr. 1941.

Ladd-Franklin, Christine, Columbia Univer., b. 1 Dec. 1847, d. 5 Mar. 1930.

Le Bon, Gustave, Paris, b. 7 May 1841. d. 14 Dec. 1931.

Leuba, James Henry, Bryn Mawr Coll., b. 9 Apr. 1868, d. 8 Dec. 1946.

Lévy-Bruhl, Lucien, Paris, b. 10 Apr. 1857, d. 12 Mar. 1939.

Lewin, Kurt, Mass. Inst. Tech., b. 9 Sept. 1890, d. 12 Feb. 1947.

Lindley, Ernest Hiram, Univer. Kansas, b. 2 Oct. 1869, d. 21 Aug. 1940.

Lindworsky, Johannes, Prague, b. 21 Jan. 1875, d. 9 Sept. 1939.

Link, Henry Charles, Psych. Corp., b. 27 Aug. 1889, d. 9 Jan. 1952.

Lipmann, Otto, Berlin, b. 6 Mar. 1880, d. 7 Oct. 1933.

Lipps, Gottlob Friedrich, Zürich, b. 6 Aug. 1865, d. 9 Mar. 1931.

MacCurdy, John Thomson, Cambridge, Eng., b. 18 Jan. 1886, d. 1 July 1947.

MacDougall, Robert, New York Univer., b. 12 June 1866, d. 1 Nov. 1939.

McDougall, William, Duke Univer., b. 22 June 1871, d. 28 Nov. 1938.

McGeoch, John Alexander, Univer. Iowa, b. 9 Oct. 1897, d. 3 Mar. 1942.

Marbe, Karl, Würzburg, b. 31 Aug. 1869, d. 2 Jan. 1953.

Marston, William Moulton, New York City, b. 9 May 1893, d. 2 May 1947.

Martin, Lillian Jane, San Francisco, Calif., b. 7 July 1851, d. 26 Mar. 1943.

Maxfield, Francis Norton, Ohio State Univer., b. 29 Aug. 1877, d. 10 Nov. 1945.

Mead, George Herbert, Univer. Chicago, b. 27 Feb. 1863, d. 26 Apr. 1931.

Meyer, Adolf, Johns Hopkins Univer., b. 13 Sept. 1866, d. 17 Mar. 1950.

Miner, James Burt, Univer. Kentucky, b. 6 Oct. 1873, d. 24 Mar. 1943.

Monakow, Constantin von, Zürich, b. 4 Nov. 1853, d. 19 Oct. 1930.

Morgan, Conwy Lloyd, Bristol, b. 6 Feb. 1852, d. 6 Mar. 1936.

Morgan, John Jacob Brooke, Northwestern Univer., b. 23 Aug. 1888, d. 16 Aug. 1945.

Müller, Georg Elias, Göttingen, b. 20 July 1850, d. 23 Dec. 1934.

Myers, Charles Samuel, London, b. 13 Mar. 1873, d. 12 Oct. 1946.

Patrick, George Thomas White, Univer. Iowa, b. 19 Aug. 1857, d. 21 May 1949.

Pavlov, Ivan Petrovitch, Leningrad, b. 26 Sept. 1849, d. 27 Feb. 1936.

Pearson, Karl, London, b. 27 Mar. 1857, d. 24 Feb. 1936.

Peterson, Joseph, Peabody Coll., b. 8 Sept. 1878, d. 20 Sept. 1935.

Pintner, Rudolf, Columbia Univer., b. 16 Nov. 1884, d. 7 Nov. 1942.

Poppelreuter, Walther, Bonn, b. 6 Oct. 1886, d. 11 June 1939.

Prince, Morton, Boston, Mass., b. 21 Dec. 1854, d. 31 Aug. 1929.

Pyle, William Henry, Wayne Univer, b. 27 Feb. 1875, d. 3 Mar. 1946.

Rank, Otto, New York City, b. 22 Apr. 1884, d. 31 Oct. 1939.

Ranschburg, Paul, Budapest, b. 3 Jan. 1870, d. 18 Jan. 1945.

Richet, Charles, Paris, b. 26 Aug. 1850, d. 3 Dec. 1935.

Rignano, Eugenio, Milan, b. 31 May 1870, d. 9 Feb. 1930.

Robinson, Edward Stevens, Yale Univer., b. 18 Apr. 1893, d. 27 Feb. 1937.

Robinson, Florence Richardson, New Haven, Conn., b. 3 July 1885, d. 3 Dec. 1936.

Rosanoff, Aaron Joshua, Sacramento, Calif., b. 26 June 1878, d. 7 Jan. 1943.

Rubin, Edgar John, Copenhagen, b. 6 Sept. 1886, d. 3 May 1951.

Ruch, Giles Murrel, Washington, D. C., b. 7 July 1892, d. 15 Nov. 1943.

Sachs, Hanns, Boston, Mass., b. 10 Jan. 1881, d. 10 Jan. 1947.

Saudek, Robert, London, b. 21 Apr. 1881, d. 15 Apr. 1935.

Schiller, Paul Harkai, Yerkes Lab. Primate Biology, b. 4 Nov. 1908, d. 1 May 1949.

Schumann, Friedrich, Frankfurt a.M., b. 16 June 1863, d. 10 Mar. 1940.

Seashore, Carl Emil, Univer. Iowa, b. 28 Jan. 1866, d. 16 Oct. 1949.

Seashore, Robert Holmes, Northwestern Univer., b. 14 June 1902, d. 27 Aug. 1951.

Shand, Alexander Faulkner, London, b. 20 May 1858, d. 7 Jan. 1936.

Sharpey-Schafer, Edward Albert, Edinburgh, b. 2 June 1850, d. 29 Mar. 1935.

Shepherd, William Thomas, Columbia Univer. Sch., Washington, D. C., b. 4 Feb. 1867, d. 2 Feb. 1945.

Sherrington, Charles Scott, Oxford, b. 27 Nov. 1857, d. 4 Mar. 1952.

Shirley, Mary Margaret, Indiana Univer., b. 7 Feb. 1899, d. 11 June 1946.

Small, Willard Stanton, Univer. Maryland, b. 24 Aug. 1870, d. 31 Jan. 1943.

Smith, Stevenson, Univer. Washington, b. 29 Apr. 1883, d. 27 Nov. 1950.

Snoddy, George Samuel, Indiana Univer., b. 21 Apr. 1882, d. 29 June 1947.

Spearman, Charles Edward, London, b. 10 Sept. 1863, d. 17 Sept. 1945.

Starbuck, Edwin Diller, Univer. Southern California, b. 20 Feb. 1866, d. 18 Nov. 1947.

Stern, William, Duke Univer., b. 29 Apr. 1871, d. 27 Mar. 1938.

Stetson, Raymond Herbert, Oberlin Coll., b. 1 Mar. 1872, d. 4 Dec. 1950.

Stoelting, Christian H., Chicago, Ill., b. 18 July 1864, d. 18 Mar. 1943.

Stout, George Frederick, St. Andrews, b. 6 Jan. 1860, d. 18 Aug. 1944.

Stumpf, Carl, Berlin, b. 21 Apr. 1848, d. 25 Dec. 1936.

Sutherland, Arthur Howard, Louisiana Coll., b. 19 Nov. 1878, d. 18 May 1951.

Swift, Edgar James, Washington Univer., b. 24 July 1860, d. 30 Aug. 1932.

Tait, William Dunlop, McGill Univer., b. 20 Nov. 1879, d. 10 May 1945.

Thorndike, Edward Lee, Columbia Univer., b. 31 Aug. 1874, d. 9 Aug. 1949.

Triplett, Norman, Kansas St. Teach. Coll., Emporia, b. 1 Oct. 1861, d. 16 Oct. 1934.

Troland, Leonard Thompson, Harvard Univer., b. 26 Apr. 1889, d. 27 May 1932.

Twitmyer, Edwin Burket, Univer. Pennsylvania, b. 14 Sept. 1873, d. 3 Mar. 1943.

Valentine, Willard Lee, Northwestern Univer., b. 2 Dec. 1904, d. 5 Apr. 1947.

Warren, Howard Crosby, Princeton Univer., b. 12 June 1867, d. 4 Jan. 1934.

Washburn, Margaret Floy, Vassar Coll., b. 25 July 1871, d. 29 Oct. 1939.

Weiss, Albert Paul, Ohio State Univer., b. 15 Sept. 1879, d. 3 Apr. 1931.

Wembridge, Eleanor Harris Rowland, Los Angeles, Calif., b. 9 Dec. 1883, d. 20 Feb. 1944.

Wertheimer, Max, New School for Social Research, b. 15 Apr. 1880, d. 12 Oct. 1943.

Wheeler, William Morton, Harvard Univer., b. 19 Mar. 1865, d. 19 Apr. 1937.

Whipple, Guy Montrose, Clifton, Mass., b. 12 June 1876, d. 1 Aug. 1941.

White, William Alanson, St. Elizabeth's Hosp., Washington, D. C., b. 24 Jan. 1870, d. 7 Mar. 1937.

Willoughby, Raymond Royce, Providence, R. I., b. 20 Apr. 1896, d. 3 Oct. 1944.

Wirth, Wilhelm, Leipzig, b. 26 July 1876, d. 13 July 1952.

Woolley, Helen Bradford Thompson, Columbia Univer., b. 6 Nov. 1874, d. 24 Dec. 1947.

Yoakum, Clarence Stone, Univer. Michigan, b. 11 Jan. 1879, d. 20 Nov. 1945.

Yule, George Udny, Cambridge, Eng., b. 18 Feb. 1871, d. 26 June 1951.

Zwaardemaker, Hendrik, Utrecht, b. 10 May 1857, d. 19 Sept. 1930.

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Received March 31, 1953.

THE PSYCHOLOGICAL BULLETIN
Vol. 51, No. 1, 1954

SPECIAL REVIEW[1]

TEXTBOOKS AND GENERAL PSYCHOLOGY

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ASHER, ESTON J., TIFFIN, JOSEPH, & KNIGHT, FREDERIC B. *Introduction to general psychology.* Boston: Heath, 1953. Pp. xvi+515. $4.25.

BUXTON, CLAUDE E., COFER, CHARLES N., GUSTAD, JOHN W., MACLEOD, ROBERT B., MCKEACHIE, WILBERT J., & WOLFLE, DAEL. *Improving undergraduate instruction in psychology.* New York: Macmillan, 1952. Pp. vii+60. $1.25.

COLE, LAWRENCE E. *Human behavior: Psychology as a bio-social science.* Yonkers-on-Hudson: World Book Co., 1953. Pp. xi+884. $5.50.

GUILFORD, J. P. *General psychology.* (2nd Ed.) New York: Van Nostrand, 1952. Pp. xii+587. $5.00.

HILGARD, ERNEST R. *Introduction to psychology.* New York: Harcourt, Brace, 1953. Pp. x+659. $5.75.

SKINNER, B. F. *Science and human behavior.* New York: Macmillan, 1953. Pp. x+461. $4.00.

STAGNER, ROSS, & KARWOSKI, T. F. *Psychology.* New York: McGraw-Hill, 1952. Pp. xiii+582. $5.00.

The temerity of the textbook writer who believes that he has produced the answer to general psychology's prayer is exceeded only by the audacity of the reviewer who presumes to pass judgment on the belief. More rash than wise is the individual who would go still further and attempt an evaluative comparison of several current texts. It may be fun to predict consumer reaction, or to dare some hardy instructor to edge himself out on an untested textual limb. But a more helpful function of the review is to present the new books in broad context, leaving the responsibility for evaluation where it belongs—with each individual teacher. In view of the theoretical differences that characterize psychology, and the diversities in philosophy and practice typifying the teaching profession, it would be folly to expect that any teaching aid could be all things to all pedagogues.

If there is any discernible trend running through all higher education today, it is a healthy compulsion to self-examination. It is interesting to speculate that its temporal proximity to World War II and continuing international crisis is more than coincidental. Manpower problems are more obviously critical, the success of selection and training methods in the armed services suggests that parallel problems in the academic field might yield to equivalent experimental analysis, and perhaps there is impatience with a traditional scholarship that has failed to open the road to Utopia. Whatever the explanation may be, the consequence is clear. Spurred on by Presidential commissions and educational foundations, every college institutes some new curricular reform, or at least renames an old one; private and public soul-searching is the order of the day.

In view of psychology's subject

[1] This is the second special review dealing with the evaluation of books in a given area of psychology (cf. Editorial Note, *Psychol. Bull.*, 1953, **50**, 149).

matter and demonstrated skills, it would be anomalous indeed if we were unwilling to confront the situation in our own bailiwick. While much of our extra-laboratory energy has been applied to clinical and industrial problems, curriculum rationale and teaching procedures have been receiving increasing amounts of attention. It is becoming gradually accepted that graduate departments may with profit organize seminars to guide their students in the development of instructional proficiency. The Division on the Teaching of Psychology is growing not only in membership but in productive activity. Most important, the recognition is spreading that criteria are fully as elusive in the classroom as in pilot training, that sampling biases are not automatically eliminated by college matriculation, and that pedagogical preconceptions, like experimental hypotheses, exist to be challenged.

What's Wrong with General Psychology?

The focus of discussion has been the introductory psychology course. It might be argued that the disproportionate importance of this course makes it worthy of greatest effort, for it serves the greatest number of students and it is the primary medium of recruitment. Further, it probably encompasses the problems of all but the most technical courses, both undergraduate and graduate. And, if further excuse is needed, the beginning is the logical place to begin. What shall be our objectives in this course: to increase the student's appreciation of the advantages and limitations of scientific method, to nourish the development of more objective attitudes toward human behavior, to enhance general understanding of behavior dynamics by delineating basic psychological principles, to give a balanced view of what constitutes the scientific discipline and the professional practice called psychology, to help the student solve his emotional and vocational problems? Should we lecture to our classes, or plan directed discussions, or just sit and exude warm acceptance? Is the major assigned reading to repeat the class activities or is it to cover the topics for which no class time is available? Is the number of pages apportioned to the several topics to reflect the relative interest that a sample of unsophisticated students have expressed? Is the reading to furnish case material for group dissection? Is it to add the living flesh of research to the bare skeleton of topics orally presented? Is it to be entertaining and lively, or sober and pedestrian?

A Guide: Buxton et al.

A partial set of answers comes from the symposium on the improvement of undergraduate psychology instruction, held at Cornell during the summer of 1951. Much of the report deals with the introductory course. It is first assumed that the desires and expectations of the students should not dictate the nature of the course, although they may well be exploited to facilitate the learning process. Problems of everyday interest may furnish the point of departure, but the instructor must be sure to depart a perceptible distance. This is definitely not to be a personal adjustment course, and the intellectual goals must never become secondary to the urge to conduct group therapy. The primary teaching emphasis in the ideal curriculum is to be upon knowledge and content (the problems of psychology, facts

and principles of psychology, psychology as science, the structure and functioning of science), which should then indirectly lead the student toward the objectives of rigorous habits of thought and modified attitudes toward behavior.

The study group finds that the effectiveness of the usual introductory text is impaired by the unfortunate combination of two divergent organizations, and proposes that either the developmental or the cross-sectional approach be explicitly adopted and rather strictly followed. In the sample outlines presented, less than the traditional emphasis is placed upon the topics of neural anatomy and physiology, the receptors, the effectors, tests, individual differences, and statistics. While no selection and arrangement of topics can remain definitive in a changing field, the adoption of some guiding organization is preferable to a listless eclecticism born of inertia and ancestor worship.

Science and Practical Living: Hilgard

Hilgard's basic orientation involves two major objectives, which at first glance may seem to be incompatible. He wishes the text to give a fair view of psychology as a whole, and to introduce the student to "those topics which are the centers of excitement among professional psychologists today" (p. ix). At the same time, he recognizes as valid the desires of the elementary student to receive help in increasing self-understanding. To reconcile these aims is not impossible, he feels. After all, more psychologists are actively concerned with industrial and clinical matters than with retinal interaction. And so it is fair enough to devote nearly 20 per cent of the book to a section entitled "Psychology Applied to Personal and Social Problems" ("Mental Health and Readjustment Techniques," "Vocational Adjustment," "Psychology in Industry," "Public Opinion and Propaganda," "Problems of Social Groups"), while omitting the usual chapters on receptor structure and processes, the nervous system, biological genetics, and statistics. Still more significant is the impression that Hilgard is striving to be "person-oriented" in the other 80 per cent, most obviously so in the chapters on "Adolescence and Adulthood," "Emotion and Motivation," "Social Motives," "Conflict and Frustration," and "Individual Modes of Adjustment." He is not simply describing a series of representative experiments from the relevant sections of *Psychological Abstracts*. He is instead analyzing some major problems of behavior in terms understandable to the student, and recounting what he has come to think about them after all these years as a psychologist; experiments are interpolated chiefly to indicate that his thinking is not entirely autistic.

If relegation of the laboratory to the relatively minor position will please the student, it will just as surely disappoint some instructors. This is first of all a course in *science*, chiefly *experimental* science. Is it possible to preserve the spirit of research in a 16-half-line abbreviation of a 32-page experimental article? Can six or seven pages toward the end of the book, formally describing scientific procedure, adequately acquaint the student with the fundamental problems and limitations of experimental methodology and inculcate in him the desired habits of rigorous thinking? Will not the essay form, the soft pedaling of controversial data, give a false impression of ease and certainty in scien-

tific discovery? Is it enough merely to state the final polished results of investigations? What of the endless struggle with experimental controls and the slow refinement of quantitative measurement? Or can we assume that a competent instructor will be an indispensable half of the course? (It is almost paradoxical that of the four really new texts reviewed, this is the one that least needs a constant interpreter and fellow discussant, with its delightfully readable and yet mature style.)

A Unifying Concept: Stagner and Karwoski

It is often suggested that the best preparation for the Ph.D. comprehensive examination is to study an introductory text. Stagner and Karwoski's *Psychology* is admirably suited for this use. They make no attempt to be easy, to skip over difficult concepts, to subordinate the standard subject matter of psychology to matters of immediate practicality, or to "write down." While this may satisfy the graduate student's needs, it may by the same token discourage the sophomore. If the services of the professor are required to add rigor to Hilgard they will be helpful here to assure understanding and everyday interest. The one-semester course based on this text can be very solid but will surely not be easy.

A frequently voiced complaint is that general psychology is a loose stringing together of disparate topics. It is difficult for the instructor to depict for the student an integrated organism unless some unifying concept is invoked. It is to homeostasis that Stagner and Karwoski turn to fulfill this function. Some degree of success is achieved by this device. But the tying together of topics is accomplished still more meaningfully in another fashion. There is laudable willingness to mention a concept outside the chapter or the chapter section where it "belongs." Thus, the longish chapter labeled "Perception" involves considerations of personality, motivation, social factors; and perception recurs in the discussion of problem solving, of remembering, of thinking, of intelligence, of personality. Such contextual cross-referencing of course fosters continuing expansion of the meaning of each idea. Other writers, as well as teachers, would do well to extend this demonstration of the basic integrity of the behaving organism.

Revisions: Guilford; and Asher, Tiffin, and Knight

These two books, based as they are on earlier publications, represent the least drastic shift from the established pattern of the past twenty or thirty years. They will perhaps fit most readily into the traditional course organization.

In 1941 Guilford was reported a best seller. Unless your favorite experiment is among those pruned away, there is little reason for you to like this blue printing any less than the red printing. A dozen pages have been added on "What Psychologists Do," there is a new 30-page section on heredity and development, and the rest of the material is presented in somewhat modified order.

Asher, Tiffin, and Knight, in what is essentially its third edition, has shifted its center of gravity somewhat toward the right—slightly farther removed from the strictly applied and closer to psychology as science. Of the 14 chapters in this relatively short book three are new ("Development of the Individual," "Body Structures and Behavior,"

and "Motivation"), and others have been partially rewritten.

A Cross-Discipline Approach: Cole

It is not always easy to judge whether an author's second book in a field is different enough from the first to require additional shelf space. A simple criterion now suggests itself: if another publisher is involved, any similarity between the two is not only coincidental but probably illegal! *Human Behavior* represents a considerable departure from Cole's earlier *General Psychology* (**2**) and from all other elementary psychology texts, in both emphasis and organization.

The most startling difference is found in the concluding section, 200 pages devoted to the "self-system." After a chapter relating the hypnotic state and the divided self, there are five chapters describing major aspects of psychoanalytic theory. The final chapter, "The Normal Personality," is a 50-page discourse which, as Cole suggests, goes beyond psychology in answering the "So what?" of the entire course. The reader will look in vain for the usual discussion of types vs. traits, an outline of personality tests, and the classification of deviations. Instead he will find an original dissertation on the philosophical and practical meaning of "normality," with an unusually varied selection of allusions to literature, theoretical psychiatry, and theology. It will be a strong dose for the student to swallow as he comes to the end of his survey of psychology. The average instructor will have to be shaken loose from a great many scientific fixations if he is to regard the treatment as nourishing rather than purgative.

The second distinguishing feature is the abundance of documentation throughout the book, drawing freely and at length from experimental descriptions, philosophical treatises, sociological and anthropological expositions, and a wide variety of case reports. If Hilgard tends to use illustrations as footnotes to his discussion, and if Stagner and Karwoski incorporate their references more intimately into the textual material, Cole can be said in some chapters to be providing the interpretive continuity for an extended anthology of behavior stories. The average beginning student will find that a great deal of the material makes interesting reading, but he will probably also find himself in need of considerable classroom guidance if he is to abstract the pattern of principles usually considered to constitute the core of general psychology. In a very real sense, we have another difficult text.

Surprise Entry: Skinner

Inclusion of this item in the review may be unexpected and even controversial. Admittedly it would be impossible if the list were restricted to books deliberately designed for the general psychology course or to those deviating no more than one sigma from the current norm. But even if it is unacceptable for widespread classroom adoption, its consideration should force us to scrutinize a little more closely whatever faith underlies our practice in the first course.

Science and Human Behavior was prepared in connection with the Harvard general education course in behavior science. The unhappy condition of the world is attributed to a failure to resolve, or even to face, a basic contradiction between opposed conceptions of man. "A scientific conception of human behavior dic-

tates one practice, a philosophy of personal freedom another" (p. 9). A wise choice presupposes understanding of the alternatives; it is Skinner's responsibility to clarify the first position, by providing a meaningful definition of science, by demonstrating that human behavior is amenable to the manipulations of experimental science, and by considering the implications for practical living that must inevitably emerge. His belief that the variables significant for the understanding and control of behavior lie only in the immediate environment and the behavioral history of the organism is illustrated in 150 pages of relatively nontechnical and nonquantitative discussion of operant behavior. Where the laboratory demonstration involves pigeons—and it not infrequently does—logically parallel examples from common human experience are proposed. For example, the cumulative effect of accidental contingencies in operant feeding reminds us of human superstition, the unusual efficacy of certain reinforcement schedules seems to be relevant to the optimal payoff rate of gambling devices, verbal responses are modified under the influence of discriminative stimuli, imitative birds have their counterpart in human dancers, punishment temporarily suppresses the rate but leaves unchanged the rat's or child's total number of responses. Throughout this section, the systematic position of the writer is never obscure. (Tip to the graduate student: these chapters should be reprinted as "Skinner Almost Painlessly Revealed.")

To reinforce the premise that complex behavior is within the potential range of science, the principles of simple cases are then extended to self-control, to thought processes and inner events, and to the functionally unified system of responses sometimes called "the self." The social situation is brought within the same framework, beginning with the two-person relationship and relentlessly (perforce speculatively) driving on to the controlling agencies of government, religion, psychotherapy, economics, and education. Like Cole, Skinner recognizes that when the science of behavior is pushed to its ultimate, the problem of who is to control whom and for what ends runs squarely up against some of the most cherished conceptions of human life. He does not counsel retreat.

How does this help us in introductory psychology? How can we develop in our students a respect for psychology as a quantitative and controlled mode of investigation if we omit the details of experimental design and procedure, if we fail to talk in terms of means and deviations? Can anything but a distorted view result from trying to survey modern experimental psychology from the confines of the Skinner box? What happens to all the time-honored observations and principles that we have come to know as general psychology?

Perhaps the answer is that these facts and generalizations are unimportant and soon forgotten, that clear perspective can most economically be gained from a consistent vantage point, and that numbers and gadgets are not the essence of science. Surely the story of the science-behavior marriage in the typical text is by contrast to this exposition pale and unsubstantial, the attempt to relate psychology to other aspects of life pitifully tentative. For those who feel that this part of our job is the most important, a little more of the Skinnerian approach would seem to

be in order. If the unsupplemented classroom use of *Science and Human Behavior* is precluded by limitations of curriculum and clientele, its assimilation by the serious teacher is most appropriate.

Trends

Does the curve pass through enough points to permit extrapolation?

To summarize the contemporary trends in the general psychology course would require a very complex formula indeed, with about as many negative terms as positive. And yet some hints may emerge from a comparison of the present set of texts with a somewhat earlier reference group, such as Woodworth and Marquis (**6**), Dashiell (**4**), Cruze (**3**), and Munn (**5**).

Topics. The most obvious difference is in the line-up of chapter and section headings. There seems to be a tendency, though not unopposed, to transfer the formal study of statistical techniques and the organic bases of behavior to advanced courses or to other departments. Thus statistics covers a total of perhaps six pages in the six books; the nervous system as a separate topic appears only in the two revisions; genes have all but disappeared from the new books; Hilgard and Cole find it possible to write general texts without chapters on the receptors. With the addition of development sections to the revisions, only Stagner and Karwoski deviate in this respect from the reference texts. And yet any listing of labels or counting of pages must be viewed with suspicion; for example, it is doubtful that any of the books with the development chapter qualify as pure representatives of the "developmental" orientation outlined by Buxton *et al.*, and it would be incorrect to assume that any but Skinner would exclude neurophysiological variables from psychology. It is more nearly correct to say that a trend away from compartmentalization has blurred certain arbitrary distinctions and encouraged the juxtaposition of items formerly separated in writing. So, Stagner and Karwoski interject a discussion of significant characteristics of the nervous system at any point where it may clarify other topics, and Cole calls his approach *bio*social in spite of his chapter omissions.

While convincing quantitative proof may be lacking, I have the impression that more emphasis is now being placed upon personality, and especially upon its social determinants. The self concept has come into prominence (Hilgard, Stagner and Karwoski, Cole, Skinner) and there is no question but that the insights of psychoanalysis are enjoying a much more general acceptance. This, together with Hilgard's and Guilford's chapters surveying the fields of psychological endeavor, may suggest that general psychology, like the APA, is becoming concerned with the profession as well as the science.

The whimsical order of topics has traditionally been a source of variety to lighten the yearly chore of teaching introductory psychology. There is in the present sample good evidence that some measure of stability is being approached—at least, development and motivation come early, and personality late.

Specific content and format. More deadly to the chronic instructor than a fixed list of chapters is the same diet of illustrative stories, experiments, and pictures regurgitated from one generation of writers to the next. The student may be unaware

of the difference, but the jaded teacher will be refreshed by some of the new dishes cooked up by the current authors. Hilgard has been the most resourceful in the search for new ingredients to satisfy old recipes, and Stagner and Karwoski's chapter on thinking has a very liberal sprinkling of original elements.

Generosity in the use of pictorial material, as well as variation in size and arrangement of page, is at once an economic, an aesthetic, and a psychological problem. Some publishers must recognize that the principles of satiation and of diminishing returns are as pertinent as the principles of attention. It may be for this reason that the use of half-tones is quite limited in Cole and in Asher *et al.*, and moderate in Stagner and Karwoski, and Guilford. Hilgard, on the other hand, has what might be called a superabundance, and distraction begins to outweigh clarification. It is probably helpful to portray in action representatives of three extreme somatotypes, and one well chosen picture may be more valuable than three hundred words analyzing depth perception, but it is questionable that a picture of ten giraffes or a ship under construction will contribute a half-page worth to the student's understanding of gregariousness or demoralization. The very obvious trend of increasing prices might be decelerated without educational loss if the criteria of moderation and relevance were more prominent in the determination of form.

Passing mention should be made of the excellent bibliography furnished in Hilgard. It contains upwards of 850 items, given in complete form and keyed to the pages in the text on which reference is made. There is also a glossary of about 500 terms.

Systematic bias. There have been very few elementary texts with the controversial flavor of Titchener and Watson, and our first five suggest no unique trend in this regard. Few teachers are blessed with systematic conscience so strict as to prohibit assignment of any of the chapters (except perhaps Cole's critical exposition of Freudianism), although as usual there will be some choking over definitions, distress at internal inconsistencies, and puzzlement as to how experience is to be examined if the instrument of introspection is discarded. Skinner is of course not so universally inoffensive. With all its restrictive disadvantages, this at least can be said of such a persistent theoretical treatment—that the patient knows he has been treated. When the student finishes Skinner (or vice versa), he will be aware that he has been up against something, whether good or bad. Too few college experiences can similarly be characterized.

Teaching aids. It would seem logical that the instructor is in the best position to decide how a textbook and other supplementary devices can optimally be fitted into the course plan. However, as a guide for the inexperienced or preoccupied and as a source of new ideas, many writers furnish extratextual material. Guilford, and Asher *et al.*, include self-test questions after each chapter. Separate manuals for instructor and student accompany both Hilgard, and Stagner and Karwoski. Their quality indicates that greater than usual effort was expended in their preparation. Incidentally, it is in the Stagner and Karwoski material that we find the only suggestion that laboratory work can be a useful part of the introduction to psychology.

The Moral

No great perspicacity is required to sense the restlessness that pervades instruction in general psychology. We can't quite agree on what we should be doing, or how. The problem certainly isn't solved when in grammatical confusion we echo the old wheeze about teaching students instead of psychology. The report of the Cornell symposium does not supply the final word (cf. **1**). Nor should we expect any textbook writer to solve the riddle for us. It is not enough, for example, to adopt a text whose preface solemnly asserts that it is "student oriented" and which thereafter departs from the old mold only by addressing the reader as "you" instead of "one."

The answers must be found in the course itself, not in the auxiliary aids provided. There are a dozen texts that are still adequately up to date and that can be used with profit by the capable class and the thoughtful instructor. No one of them can assume the responsibility for teaching. (To add to the cliché, you can't "teach a book," either.) Indeed, if a choice were forced as to which locus of student-orientation is the more critical, it should obviously be the day-by-day activity of the instructor, with the printed supplement simply a sound and representative description of psychological science. There are many ways of teaching effectively; it is up to each instructor to generate some hypothesis as to what procedures and what adjuncts are most fruitful, and then test and revise and retest the hypothesis *ad infinitum*. The diversity of textbook offerings is most fortunate, if it has the effect of forcing the instructor into a questioning and experimental frame of mind. The crucial self-examination in higher education is after all at the level of the individual.

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Received July 7, 1953.

HULL, CLARK L. *A behavior system: an introduction to behavior theory concerning the individual organism.* New Haven: Yale Univer. Press, 1952. Pp. ix+372. $6.00.

In the preface to this book, written shortly before his death, Professor Hull places it as the second in a three-volume plan. The first volume, *Principles of Behavior*, appeared in 1943. He writes that it was designed "to state the more important primary principles considered necessary to mediate the deductions of a natural-science theory of behavior." The intent of the present work, as the second volume, is "to show the application of the principles to the deduction of the simpler phenomena characterizing the behavior of single organisms." The plan called for a third volume which would "apply these same principles to the deduction of the elementary phenomena of social behavior, i.e., of behavior manifested when the interacting objects are mammalian organisms of the same species." Professor Hull foresaw that he would probably not live to write the third volume. In some sense this second volume is his final bequest to the science of psychology. As such, it deserves sober review as the crowning achievement of more than 20 years devoted to this type of systematic formulation.

In fairness to Professor Hull, it must be remembered that this work was written during his years of failing health, when he could work but a few hours a day, and when he could not rely, as formerly, on an active and continuing intellectual interchange with his colleagues and students. Hence the critic may detect incompletenesses and contradictions which perhaps ought not to be there, yet which would not have been there had Professor Hull been able to complete this with the same painstaking care and meticulous revision which characterized his earlier published papers and books. This book should be reviewed in the spirit of one seeking to find out what can be learned from it by way of both method and content; it would be improper to concentrate upon errors of detail. Professor Hull did not believe that he had said the last word. His hope, expressed in the preface and in the conclusion, is that the method exposes its own errors, so that serious students can carry forward the work of making succeeding systems more precise.

> Those who know the history of theoretical psychology will understand that the present system is merely the most recent of a series of miniature systems evolved by the present writer. The coming generation of scientists will, it is hoped, present other theoretical systems, each succeeding one of a progressively more precise and quantitative nature (p. 353).

I shall attempt to deal, first, with some of the more ingenious substantive contributions found in the book, second, with its systematic and methodological significance, and, third, with a general estimate of Professor Hull's place in the history of twentieth-century psychology.

Substantive Contributions

Those who were troubled by the limitations of topical coverage in *Principles of Behavior* will be pleased to find in the present volume many of the topics from the papers which appeared in the *Psychological Review* in the early 1930's, including behavior in relation to objects in space, multidirectional maze learning, the

problem-solving assembly of behavior segments.

In planning his deductive system, Hull made many early forays into more complex behavior, including the social behavior that he intended to treat in the final projected volume. But then he retraced his steps, so that, beginning with revised postulates, he could move from simple to more complex behavior. In *Principles of Behavior* he was concerned chiefly with the system of postulates, and so chose to deal quantitatively with behavior near to that formulated in the postulate system, with a minimum of what he called "multiple-link" deductions. The revised postulates were published in a little volume, *Essentials of Behavior* (1951), which chiefly brought the postulates in *Principles of Behavior* up to date as a background for the second volume. Now in *A Behavior System* he reviews, revises, and extends many of the deductions of more complex behavior which he had toyed with before the postulate system was well formulated. Hence, this new volume recovers some of the richness of the early papers which brought his work to the attention of psychologists interested in many aspects of learning remote from classical and instrumental conditioning.

As a brief introduction to some of the substantive achievements, I would direct the reader to the following six deductions, with the corresponding empirical verification:

a. Response alternation in trial-and-error learning. The reference experiment is one in which a rat can be reinforced by moving either a horizontal or a vertical bar. When both bars have been reinforced, and then a shift in reinforcement is made from the more strongly and recently reinforced bar to the other, there ensues a period of alternation before the response is given more frequently to the (now) regularly reinforced bar. The systematic relationships predicted from the theory (Figure 11, p. 48) are borne out empirically with some qualifications (Figure 15, p. 52).

b. Generalization gradients to stronger and weaker stimulus intensities. The theoretical differences in curvature of the gradients (Figure 28, p. 82) is borne out empirically (Figure 29, p. 83).

c. Latent learning. The shifts in reaction with shifts in incentive found in the early studies by Tolman and his collaborators, demonstrating latent learning, are now predicted. The prediction is shown in Figure 37, p. 144, the empirical material in Figure 38, p. 145, and Figure 39, p. 146.

d. Reaction latency at points in a behavior chain. It is predicted that for a four-unit chain, reaction latencies will decrease for three units and increase for the final unit. The prediction is shown in Figure 42, p. 163, the empirical verification in Figure 43, p. 164.

e. Effect of motivation upon behavior in a conflict between an adient and an abient object. The discussion and derivations are found on pp. 245–252, culminating in Theorems 88 and 89, both of which are verified in the empirical outcomes of Table 32, p. 253.

f. Entrances into goalward-pointing alleys vs. those pointing away from the goal, in a multidirectional maze. The prediction of greater frequency of entrances into goalward-pointing blinds is summarized in Theorem 112, p. 289, the empirical data in Table 35, p. 291.

Professor Hull has made his own computation as to agreement between prediction and empirical find-

ing. His figures are summarized in Table 1.

Curiously enough, the result would be somewhat more convincing were the score poorer, rather than better. The success is not so convincing because too many of the derivations are very close to the data predicted, representing almost a working backward from what is known.

TABLE 1

EMPIRICAL AGREEMENT WITH THEORETICAL PROPOSITIONS (From Hull, *A behavior system*, pp. 351–353)

Propositions	*Number*	*Per Cent*
All theoretical propositions		
Evidence found bearing on validity	93	52
Indirect evidence only	30	17
Subtotal	123	69
Not covered by known relevant evidence	55	31
Total	178	100
Propositions covered by direct or indirect evidence		
Substantially validated	106	86
Probably valid; considerable uncertainty	14	11
Definitely invalid	1	1
Not classified	2	2
Total	123	100

Working backwards is not, in itself, a defect in a theoretical system, provided the system is coherent and internally consistent, and provided it also is fertile in predicting relationships not yet explored. As Hull points out, the 55 predictions not covered by known relevant evidence will provide a good test, for these predictions are free of the charge of working backward from the known.

The range of coverage of the system is sufficient to test its systematic and deductive character, for the predictions cover many kinds of behavior not referred to directly in the postulates.

Methodological and Theoretical Contributions

Hull makes ingenious use of the hypothetico-deductive system, in which quantitative treatment moves back and forth between basic principles, or postulates, and empirical consequences, expressed in corollaries; no one can seriously doubt the usefulness of such a model as that which he has furnished. The method is perhaps best elaborated as an exercise in logic and mathematics in the earlier multiple-author volume entitled *Mathematico-Deductive Theory of Rote Learning* (1940). The similar approach, but with a different postulate system, in *Principles of Behavior* and now in *A Behavior System* is a model which will endure in the history of psychology.

The basic condition of learning, according to Hull, is repeated reinforcement which comes about through drive reduction. Some very important changes took place in Hull's quantitative theory of reinforcement between *Principles of Behavior* (1943) and *A Behavior System* (1952). While a complete discussion of these changes would require many pages, some of the main changes may easily be noted.

Habit strength ($_sH_R$) in the earlier book increased with reinforcement. The *amount* of increase per reinforcement depended upon (*a*) the magnitude of need reduction per reinforcement, (*b*) the delay in reinforcement, and (*c*) the interval between the conditioned stimulus and the response to be conditioned to it. In the new book habit strength is solely a function of the number of repetitions of a closely associated stimulus and response, provided this *S-R* conjunction is reinforced. The amount of reinforcement, and the time relation-

ships, no longer affect ${}_sH_R$. Hence the basic learning condition becomes, in fact, much closer to that proposed in the familiar views of Guthrie and, more strikingly, of Tolman, because the basic learning process depends upon contiguous *S-R* relationships, relatively independent of motivation. The difference is preserved in that some minimum of reinforcement must occur. (This necessary minimum is nowhere made clear in the postulates or the text.) The main advantage of this change in relation of ${}_sH_R$ to need reduction lies in the possibility of deducing latent learning—the empirical basis for Tolman's views in the first place.

What habit strength (${}_sH_R$) loses, reaction potential (${}_sE_R$) gains. The distinction between habit strength and reaction potential is essentially the distinction between learning and performance, early emphasized by Lashley and Tolman. Habit strength is energized into actual behavior only by being converted to reaction potential through the effect of drive (D), stimulus-intensity dynamism (V), and incentive motivation (K). The delay in reinforcement (J) also affects the size of reaction potential which can lead to response at some link in the behavior chain. The drive (D) was familiar in the 1943 version, but (V) is new and (K) plays a new role. The stimulus intensity dynamism (V) refers to the strength of the signalling (conditioned) stimulus. A stimulus of greater intensity will evoke a greater response, even though ${}_sH_R$ remains constant. Incentive motivation (K) refers to the quantity of incentive used in earlier trials. It affects ${}_sE_R$, but not ${}_sH_R$. (There is no reference in the postulates to the size or nature of the *perceived* incentive in adient or abient situations, an important variable in much motivated learning.)

With increasing emphasis upon secondary reinforcement, the precise differences between primary and secondary reinforcement become important. In 1943 it is said that primary reinforcement is the result of diminution of a need or drive, although elsewhere it is said that it is the drive stimulus S_D (not D) that is reduced. This latter statement is the one adopted in 1952, with the addition of the possibility that reduction in S_G (the stimulation consequence of the fractional anticipatory goal response) is reinforcing.

If stimulus termination (rather than need reduction) is the basis for reinforcement, then secondary reinforcement is more readily accounted for; it becomes, in 1952, a *corollary* of primary reinforcement rather than a separate postulate. (There are in fact two corollaries of primary reinforcement, one describing secondary motivation as the substitution of an associated neutral stimulus as the condition for a drive, the other describing how a previously neutral stimulus becomes, through association, capable of acting as a reinforcing agent.) I find a certain glibness in the announcement of these corollaries. That something of the sort occurs in learning is clear enough, but precisely what occurs is the focus of controversy between those who favor cognitive theories over reinforcement theories. Any blurring at this point is therefore unfortunate, for it leads to claims of successes in prediction through a kind of *tour de force* by which any conditions which lead to learning must necessarily involve a drive stimulus, and the reduction of the drive stimulus by an associated reinforcing agent. All that needs to be shown is a casual association of some feature of the environment

with aroused drive in the past, and of some feature with reduced drive in the past. The actual linking of stimulus and response intermediaries is often not demonstrated, only their possibilities. Such possibilities exist, of course, in any sign-significate relationship, so that sign learning appears to be easily deduced from these principles. Because only some minimal amount of reinforcement (primary or secondary) is necessary in developing habit strength, the case for a reinforcement theory is made even more plausible. This reinforcement theory requires scarcely any reinforcement, and even that little may be secondary Such a theory is not likely to be successfully refuted, even though it may be false.

It is to Hull's credit that he has searched about for formulations to take into account such demonstrated facts as came to his attention. He was willing to make quite radical modifications in his sytem, as here illustrated by the shift in properties between ${}_{S}H_{R}$ and ${}_{S}E_{R}$. He struggled with the nature of primary and secondary reinforcement. As Spence has pointed out, these modifications which make a great deal of difference in relation to certain controversies among learning theorists actually make rather little difference in the usefulness of a system of this kind, for rather simple modification in the equations commonly takes care of quite different theoretical interpretations.

There is a good deal more of straight empiricism in Hull's method than meets the eye. By comparison with some of the newer mathematical and probabilistic models it is not very mathematical at all, relying heavily as it does upon curve-fitting. This is in some ways a virtue, for these empirical relationships, if derived from careful experiment, will survive a good deal of tampering with basic assumptions about reinforcement.

Hull's Place in Twentieth-Century Psychology

Hull worked out in detail much that was implied in Watson's behaviorism. Taking Pavlov's experiments as a source of his initial postulates, he started, or gave an impetus to, a new kind of system-building in psychology. The older psychological systems, represented in the "schools" of psychology, were largely programmatic, representing ways of defining the field, classifying phenomena, selecting units, and stating certain gross "laws" or principles by which the field could be ordered. While Hull's systematic bias was that of one of these schools (behaviorism), his method was simply that of scientific logic, in which there is an interplay between empirical data and rational ways of dealing with such data. The systematic formulations lead to precise quantitative predictions, so that the system is, to some extent at least, self-correcting. This is the novelty that Hull contributed: a system at once fertile in its predictions, and precise enough to be vulnerable to experimental attack. Because he used a good deal of curve-fitting along the way, the theory has a closer affiliation with functionalism than some of the newer probabilistic models which are more rational (less *ad hoc*) than his.

There is an elegance in the effort that Hull made to develop a limited set of postulates to lead, by strict derivation, to theorems and corollaries subject to experimental test. He held up Newton's mechanics as the ideal, where a few laws of motion could account for the orbits of the planets, the tides, and the path of a projectile. He did not succeed, as

Newton did, but perhaps it is instructive to remember that Newton, too, has been superseded. Hull has set a pattern richly influencing psychology today. He has succeeded in making a permanent place for himself in the history of psychology.

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VERNON, M. D. ***A further study of visual perception.*** New York: Cambridge Univer. Press, 1952. Pp. xi+278. $7.00.

This book, although similar to the author's *Visual Perception* published in 1937, is not a revised edition but a new book. It is the nearest approximation in print to a comprehensive review of experiments on perception as traditionally distinguished from sensation. That is to say, it omits the evidence from sensory physiology, optics, and ophthalmology, and concentrates on experiments concerning the perception of form and pattern, movement, the perceptual constancies, individual differences in perception, and the facts of attention, attitude, and motivation. There is little overlap, for instance, with Bartley's *Vision*, and it covers some material omitted from Boring's *Sensation and Perception in the History of Experimental Psychology*. It might be said to be a book about *nonstimulus determinants* of perception. The experimental literature as so defined is voluminous, scattered, and contradictory. The facts as such are hard to systematize since they accumulated in the course of theoretical controversies. The author has surveyed a considerable part of this evidence by managing to cover over 500 studies in 260 pages. She has included not only the German but the French experimenters. The most valuable feature of the book may be that it will cause investigators to look up a great many references they might otherwise have neglected.

The work of Michotte and his students on the frontier between psychophysics and cognition is little understood in this country and Vernon's chapter on these experiments should prove especially useful. No comparable account exists in English. There are also good reviews of the perception of movement, of the time error in psychophysical judgment, of color constancy, and of the problems allied to attention—a word which Vernon is not afraid to use despite the controversies over its meaning.

Vernon's debt to Professor Bartlett of Cambridge is acknowledged at the outset, and her theoretical approach is allied to his. She appears to make approximately the following assumptions: first, that the perceived field is radically unlike the stimulus field—the percept is "immensely modified" as compared with the sensory stimulation; second, that sensory impulses are the raw material for perception—percepts have to be constructed; third, that the individual constructs his perceptual world in accordance with his past experience and his personality—his attitude toward an object is an intrinsic part of his perception of it. The nature of this construction process is, then, the crucial question for theory. A good many writers in addition to Vernon are struggling with this question today, especially those who hope to find in the process a key to the personality of the perceiver. So long as one assumes that perception is based on sensation it is an inescapable question. Vernon shares with other contemporaries a certain disillusion with the explanation by Gestalt organization, which seems less convincing to her than it did in 1937.

Nevertheless she, like others, uses the terminology of organization theory and has not explicitly rejected it. In the end she concludes that we do not yet understand the inner directing tendencies or schemata which shape our percepts.

The failure of the evidence to cohere, however, may be the fault of the basic assumptions under which it was gathered. Perhaps the construction process in perception is hard to discover because it does not exist. Perhaps the objective determination of perception and the subjective determination of perception cannot be mixed in a single theory of normal perception; they are incommensurable and what is required is a separate theory for each. Perhaps we must distinguish between *literal* and *schematic* perception. The reviewer has proposed a special theory of the first type but Vernon disagrees, believing that perception is always schematic, and has added an appendix defending her emphasis on subjective determination.

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SHNEIDMAN, EDWIN S., JOEL, WALTHER, & LITTLE, KENNETH B. *Thematic test analysis.* New York: Grune & Stratton, 1951. Pp. xi+320. $8.75.

Shneidman's primary purpose in *Thematic Test Analysis* was to compile a comprehensive manual of the various scoring schemes now in use for analyzing TAT stories and to demonstrate the application of such schemes to a given clinical case. A secondary purpose was to compare the relative merits of the TAT and the MAPS test for the prediction of actual behavior. To these ends he enlisted the aid of fifteen TAT methodologists, requesting them to score and interpret the TAT and MAPS stories of one John Doe. A separate chapter is devoted to each of the analyses. Behavioral data, psychiatric observations, and other test data—Wechsler-Bellevue, Rorschach, Bender-Gestalt, Minnesota Multiphasic, and Draw-A-Person—are included for the reader but were not generally available to the individual analysts. The book ends with a brief synthesis and summary by Shneidman and his collaborators, Joel and Little. The final summary chapter presents a résumé of the different approaches and their classification into five categories: normative (Cox and Sargent, Eron, Hartmann, Klebanoff), intuitive (Bellak, Holt, Lasaga, Rotter and Jessor, Symonds), interpersonal (Arnold, Joel and Shapiro, White), and perceptual (used by none as a primary technique, but by several as a secondary technique). Tabulation and quantitative analysis are left for a second book. Shneidman concludes with two "rather striking impressions" (p. 307): (1) The various clinicians show "remarkable" agreement with each other (p. 307), and "they correlate quite well with the behavioral data" (p. 303). (2) There is a commendable spirit of objective inquiry and personal humility prevailing among clinicians; they are willing "to take a position, right or wrong and to run the risk of public scrutiny" (p. 307). Neither of these conclusions will satisfy the scientifically oriented reader. To correlate "quite well" is statistically meaningless, especially in the absence of quantification. "To run the risk of public scrutiny" is nothing new to the scientist. The book in its present form serves primarily as an illustration of how various clinicians would score the same set of thematic productions.

Much of the real work of synthesis and most of the evaluation is left to the reader.

Granted that a great deal can be learned from a careful study of many different approaches to scoring thematic productions, how are we to evaluate the contributions made by this particular compilation of materials? For the student who wishes to become proficient in a given scoring system, not enough details are given by most of the analysts. Some score all of the stories, others only one or two. Moreover, many of the schemes depend so heavily on the intuition and background knowledge of the analyst that very little can be gained from the sketchy working notes which have been included. What we have here would be better termed a brief survey of scoring schemes, not a manual.

A more basic problem for evaluation centers on Shneidman's own methodology. Here the toughminded experimentalist will find much to trouble him. He could fairly object to the lack of scientific rigor in the research plan and to the plan itself. Just what, he could ask, is being examined here? Is the ultimate object of the research to test the relative usefulness of two different sets of projective materials, TAT and MAPS, for the blind diagnosis of one John Doe; or are the relative merits of the several different scoring schemes per se the major concern? Neither purpose is adequately served by the data assembled for analysis.

The data, stories told by a 25-year-old unmarried male patient to eleven TAT pictures and seven MAPS settings, were not obtained under comparable conditions. The MAPS test was administered before insulin shock therapy was undertaken, the TAT two months later by a different examiner. Behavioral data on John Doe were made available to one scorer, at her request, not to the others. What the effects of these differences are is an unknown worthy of experimental study. Yet the analysts were apparently unaware that such differences existed.

Another serious shortcoming is that individual scoring schemes were devised for the TAT, but not for the MAPS test. Many of the analysts recognize that their individual scheme is not entirely suited to the MAPS test. What profit is there, then, in forcing the stories into an ill-fitting mold? Again, most of the analysts are plagued by the absence of adequate norms for the TAT. For the MAPS they have no norms at all. Some indicate that they are attempting to establish norms for the TAT. Others are content merely to apply their own clinical knowledge to the interpretation of John Doe's stories. For communicating a scoring scheme to others, of course, this has serious limitations.

The problem of quantifying the results of the analyses is further complicated by the nature of the individual analyses since the schemes vary from fairly precise scores based on checklists (e.g., Fine) and rating scales (e.g., Eron, Hartmann, Klebanoff) to intuitive, impressionistic interpretations (e.g., Bellack, Rotter and Jessor). Some of the analysts note regretfully that Shneidman did not present the entire TAT or MAPS test for their analysis, thereby in some cases eliminating pictures which the individual analyst considered important.

And what of the "supplementary tests" and the psychiatric data which are included presumably for prediction checks? Three of the tests were given before insulin therapy,

two after it. Moreover, how are these tests really to be used? If they are to serve as validation data to evaluate the relative merits of the TAT and MAPS are we ready to accept the Rorschach or the Draw-A-Person, for example, as a validating instrument for the TAT or MAPS in the same way that we accept the Wechsler-Bellevue IQ as a measure against which to test estimates of John Doe's intellectual capacities as revealed by the TAT? These are questions which clinicians cannot answer without very precise experimental study.

On the other hand, if the psychiatric material is to be used as the validation data, here, too, Shneidman's material is poor. Five different psychiatrically trained workers interviewed John Doe from time to time. Some were social workers, others were medical men. The psychotherapy undertaken with John Doe was apparently sporadic. He seems to be one of those patients who wander in and out of hospitals and outpatient clinics, who are seen by many people for longer or shorter periods of time, and for whom no systematic treatment plan evolves.

How seriously do these methodological factors limit the usefulness of these materials? Shneidman acknowledges that they do constitute "a limitation upon the possible conclusions" (p. 4). "This investigation," he continues, "is a research but not an experiment; it employed systematic observation, not controlled experiment." This admission hardly excuses the lack of scientific rigor. To be sure, clinical settings often do set limits on a research design. John Doe often cannot be manipulated. Yet test material on John Doe accumulates in the files because of the diagnostic demands of the hospital setting. The mere availability of case material, however, does not in itself justify its extensive manipulation for publication.

One final approach to this material remains for consideration: What does it reveal about the present state of thematic tests? Taking these fifteen scoring techniques as representative of the manner in which the TAT is now being used, it is clear that much has been added to the original need-press scoring scheme of Murray and Sanford even by those who retain some of the basic structure (e.g., Aron and Holt). Yet the fundamental problem of the meaning of "projection" has not been resolved. What level of personality do these tests tap? How much effect does the stimulus (TAT picture, MAPS setting, inkblot) have on what is revealed? Are these tests primarily useful as diagnostic tools or are they better for revealing underlying dynamics? The experts do not yet agree. Most of the analysts end up with the diagnosis of schizophrenia for John Doe. Some are of the opinion that the MAPS test shows a healthier pattern than does the TAT (Klebanoff, p. 131), others find even clearer evidence of pathology in the MAPS test (Arnold, p. 36; Korchin, p. 142). Some regard the MAPS test as better for diagnostic purposes, the TAT for the dynamics. Some stress the absence of guilt (White, p. 198), others find striking evidence of guilt (Bellak, p. 51). Since all of the analysts have had considerable experience as clinicians, the differences between them stem, presumably, from differences in scoring rationale. To resolve these differences research is indeed needed. Given the present materials the reader could choose between these schemes largely in terms of personal preference for the

normative vs. the intuitive approach, for example, or for present ego-functioning vs. an approach which emphasizes reconstruction of the past. He could not choose in terms of which is the most valid technique.

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GELDARD, FRANK A. *The human senses.* New York: Wiley, 1953. Pp. x+365. $5.00.

This is a general textbook of the senses—a textbook of human sensory psychophysiology in the complete sense of these words. Its 323 pages of actual text are divided into 15 chapters. The first, 13 pages in length, is introductory. The next four, totaling 80 pages, are concerned with the sense of sight (The Visual Stimulus and the Eye; Basic Visual Phenomena; Color Vision and Color Blindness; and Visual Acuity, Contrast, and Interaction). Hearing is discussed in three chapters (Sound Energy and the Ear, Auditory Phenomena, and Electrophysiology and Auditory Theory), occupying 64 pages. The skin senses are allocated about as much space as vision and hearing, three chapters totaling 75 pages (The Skin and Its Stimuli, Pressure and Pain, Temperature Sensitivity). Kinesthetic and Organic Sensibilities, Labyrinthine Sensitivity, The Sense of Smell, and The Sense of Taste, are the headings for the last four chapters, which contain 16, 21, 25, and 29 pages, respectively. The text is followed by an excellent list of 330 references and an unusually detailed index for a book of this size. The latter is 25 pages long and must contain well over 2,000 entries.

Perhaps the most striking thing apparent in the statistical description above is the relative amount of space allocated to vision and hearing as compared with the other senses. Geldard's treatment of the former occupies a scant third of the entire book. Boring, Langfeld, and Weld (*Foundations of Psychology*), Munn (*Psychology*), and Stevens (*Handbook of Experimental Psychology*), on the other hand, assign vision and hearing the greatest prominence; in all three cases these two senses are allocated almost exactly 75 per cent of the space devoted to sensory material generally. Geldard's book, though unbalanced, represents a reasonable distribution of emphasis since there are several good textbooks concerned exclusively with vision and hearing, and scarcely any with the other senses.

As regards the general level of his writing, Geldard lies between Boring, Langfeld, and Weld, or Munn, on the one hand, and Stevens, on the other. *The Human Senses* contains considerably more advanced and detailed material on the senses than one finds in the typical good introductory text, but it is much less exhaustive and scholarly than Stevens' work. Taking Boring *et al.* and Stevens as the two ends of a scale, I should say that Geldard lies closer to the former than the latter. In fact, the chances are that the experimental psychologist who has kept reasonably abreast of recent developments will find Geldard's treatment a little too elementary for his taste.

Geldard's exposition is lucid throughout and his position is generally eclectic as regards sensory theories. I found no glaring errors to comment upon, or outrageous points of view to quarrel with. My chief complaint is rather that he committed too many errors of omission. However, if you take this book for what it is—an intermediate-level

text with a sound, general perspective—you will have to agree that it fills a definite gap in the psychologist's library.

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NORTHWAY, MARY L. *A primer of sociometry.* Toronto: Univer. of Toronto Press, 1952. Pp. vi+48. $2.25, cloth-bound; $1.50, paper-bound.

A sociometric test measures the spontaneous choices of the members (or would-be members) of a group for and against associating with one another. Obviously, it must therefore employ an appropriate criterion for the choosing. Thus, in work groups, this would be "to be in the same work unit," "as neighboring workers on an assembly line"; in hospitalized groups, this could be "to be on the same ward," etc. Once the patterns of interpersonal response are uncovered, the meaning of the choices for the individual himself, for the functioning of the group, for the importance they have in the areas of social and clinical psychology, sociology, and anthropology, are the concern of sociometry—namely, under what conditions of group life and the personal past life of individual members will such and such patterns develop. The intricacies of the patterns and the complexities of their theoretical implications are enough to make many a psychologist hesitate to pursue sociometric research persistently. It is to all workers, as well as to beginners, in this position that Northway's *A Primer of Sociometry* is addressed, and is required reading.

The book orients the beginner to the nature and role of sociometry and provides the advanced worker with perspective of what has been accomplished and what lies ahead as untouched territory for investigation. It achieves these ends in 48 pages of refreshing, clear, and compact writing. The need for such a publication is widely felt and this one bids fair to become a classic of its kind in the midst of a literature which over the last 25 years contains about as many unreliable as dependable reports.

Well-balanced and critical discussion is directed upon the merits and limitations of sociometric method, analysis, and evaluation of results. To this reviewer, the only unimportant or irrelevant discussion for the purposes of the presentation appears to be the brief use of comparisons with other methods. Practically every major aspect of sociometric research (with the omission of negative choice or rejection) is treated, followed by specific references to the most authoritative works bearing upon that aspect. Many varieties of Canadian, French, and American work are described in context under the particular aspect they shed light upon. The presentation is organized in relation to theory, practice, and application in research design. Problems are outlined which raise our sights on research into what is growth and what is mental health.

Last, not least, the author gives profoundly suggestive treatment to elements which appear likely to remain universally important to choice reactions between people. Study directed toward them may be fruitful for many generations. Hence, *A Primer of Sociometry* can be seen as a practically useful and theoretically significant research guide to students of social, emotional, and group processes.

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WEINLAND, JAMES D. AND GROSS, MARGARET V. *Personnel interviewing*. New York: Ronald, 1952. Pp. vii+416. $6.00.

Personnel Interviewing by Weinland and Gross is not likely to receive much attention from psychologists. The authors have not approached their goal of having the book ". . . serve as a guide for all those who are concerned with the selection of personnel and the maintenance of sound personnel relations" (p. iii). This text seems to be aimed at individuals in the personnel field who have had no training in psychology and have no intention of getting such training.

The authors have taken upon themselves all-inclusive and ambitious purposes: "In recent years, scientific research has made available to supervisors of interviewers and personnel managers many new techniques and applications of personnel interviewing. Some of these, such as the non-directive, group, board and stress interviews and the Chappel [*sic*]-Chronograph have been used successfully in industrial personnel work. A major purpose of this book is to explain these practical and valuable developments within a comprehensive discussion of interviewing methods, and to show how they can be employed to advantage in firms of all sizes" (p. iii). The extent to which the authors achieve their purposes is illustrated by the fact that the entire space devoted to a discussion of the Chapple Chronograph consists of two short paragraphs, and one can hardly hope to learn much about the Chapple Interaction Chronograph from this description (p. 113).

Some statements in the book cause one to wonder upon which general psychological principles the book claims to be based: e.g., "Certain wrinkles in older people tell of repeated grimaces; others of repeated smiles. The interviewer who is interested in people and takes care to study them can often tell a good deal about a person just by looking at him" (p. 16); and "Lavish jewelry suggests an ego-centered personality . . ." (p. 272).

The authors have attempted in this one short volume to deal with individual differences, personality dynamics, motivation, directive and nondirective interviewing, tests, types of interviews, correlation, etc. The shortcomings of their treatment of the various topics is to be expected in the light of their ambitious attempt. The result will probably leave the uninformed reader in a state of confusion. For example, on page 242 the authors state: "A correlation of '0.87 ± 0.12' would indicate that in a particular experiment a correlation of 0.87 was found but that the same correlation might not be found if the same tests, or measures, were used in another case. Correlations must always be taken with a 'grain of salt,' the size of the probable error showing how much 'salt' to use." These statements must leave the reader still not knowing how much "salt" is to be applied to the correlation coefficient of "0.87 ± 0.12."

A book on personnel interviewing is needed in the industrial field. It is doubted by this reviewer that the contribution by Weinland and Gross begins to fill the need.

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